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Editor—OWSEI TEMKIN

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THE RELATION OF ANCIENT PHILOSOPHY TO MEDICINE*

LUDWIG EDELSTEIN

I realize that the subject which I propose to discuss is somewhat alien to the topics to which your studies are devoted. Yet I venture to believe, and I hope not gratuitously, that my subject will seem to you meaningful at least and worth discussing. In our day it has become a commonplace to say that philosophy, the science of man, is to be based on physical science, especially on biology, physiology, medicine, and anthropology, that "the facts of man [are] continuous with those of the rest of nature."[1] Thus, understanding and conduct are to attain a firm foundation, and we flatter ourselves with the belief that in giving so much credence to medicine in the widest sense of the term we are developing tendencies inherent in ancient, in pagan thought. Did not the Greeks who worshipped the body consider man part of nature, different to be sure from animals and the world around him, but still, like all products of nature, obedient to the eternal laws of the cosmos?

At first glance, Greek philosophy seems to bear out the correctness of

* This paper was read in 1945 before the General Seminar of the New School for Social Research in New York. Preparing it for publication I have added some references to ancient sources and modern literature and have made a number of changes; but the argument has remained substantially the same.

[1] J. Dewey, *Human Nature and Conduct* [The Modern Library], 1930, p. 12; cf. p. 324.

such a thesis. Whether we study the fragments of the Presocratics, or the writings of Plato, Aristotle, the Stoics, or the Epicureans, all these philosophers, idealistic and materialistic alike, discuss the structure of the human frame. Everywhere is man's mind seen in close relationship to his body. Various temperaments are differentiated in accordance with the variety of bodily constitutions. Shakespeare's words about Brutus, " His life was gentle, and the elements/ So mix'd in him that Nature might stand up/ And say to all the world, ' This was a man! ' ",[2] seem well to express the Greek view. Moreover, philosophical books are full of physiognomic observations. The physical appearance of the human being is used to diagnose his character. The diversity of races, their achievements in art and culture are traced to physical environment. By nature, says Plato, are the Greeks fond of learning, the Thracians and Scythians high-spirited, the Phoenicians and Egyptians lovers of money.[3] And later philosophers make similar statements. Do such views not clearly indicate that Greek philosophy must have learned from Greek medicine? For from what source other than medicine can its noted naturalism stem?

Indeed, modern interpreters are inclined to speak of an influence of Greek medicine on ancient philosophical thought. I doubt the historical adequacy of such an assumption. I think that in antiquity, philosophy influenced medicine rather than being influenced by it. Philosophical insight guided the physicians in their biological, physiological, and anthropological studies. And while in this respect, philosophers were the teachers of physicians, they even were their antagonists in the struggle for leadership in the conduct of life, sternly rebuking the physicians' ambition to prescribe to man what he ought to do. The true contribution of medicine to philosophy, I venture to suggest, lies in the fact that philosophers found in medical treatment and in the physician's task a simile of their own endeavor. The healing of diseases, as well as the preservation of health, provided an analogy which served to emphasize the validity of certain significant ethical concepts and thus helped to establish the truth of philosophy; therein consisted the most fruitful relationship between ancient medicine and ancient philosophy.

Now, as regards the alleged scientific influence of medical research on philosophical studies, one must first of all beware of imagining ancient

[2] *Julius Caesar*, V, 5, 73-75.
[3] *Republic*, IV, 435 e-436 a; cf. *Laws*, 747 d-e.

medicine in the likeness of modern medicine and must remember that medicine in antiquity was primarily a craft. The Homeric poems group the physician together with the singer and the builder among those craftsmen whom everybody welcomes, although "a leech is of the worth of many other men for the cutting out of arrows and the spreading of soothing simples."[4] Fundamentally the same situation prevailed throughout all periods of ancient history. The average physician had a certain technical proficiency; he knew medicine, as the phrase goes. He acquired his skill through apprenticeship with another physician, and when he became a master in his own right, he practiced his craft, or art, as crafts and arts are always practiced, namely, in accordance with traditional views and usages. He prescribed remedies which had proved helpful before; he took care of wounds and other surgical cases in the way in which previous generations had taken care of them. While learning his trade he was not a "student of medicine"; while carrying on his business he was not a "scientist" applying theoretical knowledge to the case at hand. On the level of common medical practice biological and physiological inquiries were neither presupposed, nor were they actually made. There was not even an obligation to get any kind of training or to study at all; anybody could set himself up as a doctor without acquiring a licence or undergoing an examination. It is therefore not astonishing that even in the second century A. D. there were physicians, at least in the country, who were unfamiliar with the difference between arteries and veins,[5] a difference that had been recognized five hundred years before. Yet they could well be successful and respected practitioners, for they relied upon experience, adroitness, and manual perfection, rather than on study or research.

It is in contrast to these craftsmen—the majority of physicians throughout Graeco-Roman civilization—that a relatively small number of medical men aspired to overcome the narrow limits of their craft. If it is characteristic of all Greek scientific achievement that it was the feat of individuals following the bent of their own minds, these doctors certainly were among the finest examples of unsolicited curiosity and delight in learning. Living mainly in the big cities of the ancient world, in the centers of culture, it was they who established the science of medicine as a thin layer over the vast body of merely technical and empirical skill. They patiently observed the courses of diseases, differentiated their various

[4] *Iliad*, XI, 514 f.; cf. *Odyssey*, XVII, 382-86.
[5] Gellius, *Attic Nights*, XVIII, 10.

types, gave aetiologies, elaborated a theory of prognostics, formulated methodical rules of treatment. Doing all this, and much more than I can mention here, some of them also realized that the study of the human body must form the basis of medicine.

I have advisedly said that only some physicians acknowledged the need for such studies, for it was not until the very end of antiquity that biology and physiology were generally considered indispensable for medicine. And whether one peruses early or late medical authors concerned with these subjects, he immediately becomes aware of the fact that they were inspired by philosophy.

Even among the so-called Hippocratic writings of the fifth and fourth centuries B. C., the oldest surviving medical books, there are some treatises which deal with the nature of the human body. The doctrines here propounded clearly are adaptations of Presocratic theories, of Heraclitus, of Diogenes of Apollonia, and of others. The "Sicilian" physicians were adherents of Empedocles and of his doctrine of the four elements. Diocles, the "second Hippocrates", was an Aristotelian. In Hellenistic times, the medical sect of the Dogmatists, emphasizing the value of anatomy and biology, defended its position by reference to the doctrines of the Dogmatic philosophers and proceeded to explain the phenomena under investigation in accordance with their systems, Platonic or Aristotelian, Stoic or even Epicurean. In the same way the Empiricists, who denied that one can detect the hidden causes of the functions of the body or that such an understanding is useful for medicine, relied on philosophical arguments. They were followers of the Academic Scepsis. When subsequent to Galen, and largely through his influence, Dogmatic medicine became the representative medical system, and knowledge of biology and physiology was at last required of all scientists, a Platonizing Aristotelianism, modified by Stoic natural philosophy, became predominant in medical theory.

Only in one instance, as far as I am aware, was the assertion made that a true understanding of the nature of man can be gained not from philosophy, but from medicine alone. Yet to the Hippocratic author of *On Ancient Medicine*, who proffered this suggestion and who was perhaps himself influenced by the scepticism of the Sophistic movement, man's nature consists in his individuality; it is the sum-total of his particular reactions to food and drink; it is that which is incomparable and varies from case to case. Like later physicians in whose discussions the prin-

ciple *individuum est ineffabile* was clearly formulated,[6] the Hippocratic writer was troubled by the problem of how the uniqueness of existing phenomena can ever be comprehended by any general theory, and he therefore rejected generalizations altogether. The doctor's insistence that the object of medicine is the individual patient, that the individuality of the phenomena constitutes their only reality, led to the formation of the Methodist school of medicine, the third of the Hellenistic sects, which, affiliating itself with the newly revived philosophy of Pyrrho, renounced the possibility of all general knowledge and professed to be guided only by the Now and Here. Physicians of a similar hue in the first century B. C. had been influential in the restoration of Pyrrho's philosophy. Thus medicine to a certain extent counteracted the Greek predilection for the typical, the negligence of the particular. From the modern point of view, this may well seem to be its most outstanding contribution to Greek thought. But medical scepticism, consistently opposing any attempt at generalizing, also made the establishment of biology or physiology as a science illusory.[7]

It is fair to say, then, that only where medicine became allied with Dogmatic philosophy, did physicians actually engage in scientific research concerning the questions of life and death, of health and disease, and that they derived the principles of their biological and physiological investigations from a more comprehensive view of the universe. Of course, I do not wish to imply that all Greek medical scientists were entirely unoriginal. Men like Herophilus or Erasistratus or Galen did not hesitate to interpret the nature of the human body in accordance with their own observations. They felt free to contradict specific philosophical doctrines and in some instances to evolve new theories. But in doing so, they philosophized, they were themselves philosophers, as in Galen's opinion the true physician should be. Even the change from animal anatomy to human anatomy, which certainly was brought about by physicians, was based by them on philosophical rather than medical argumentations.[8] Moreover, once it was admitted that the doctor must be an expert in matters concerning the structure of the human body, medical research led to the

[6] Cf. K. Deichgräber, *Die griechische Empirikerschule*, Berlin, 1930, p. 311.

[7] For the development here sketched, cf. L. Edelstein, "Empirie und Skepsis in der Lehre der griechischen Empirikerschule," *Quellen u. Studien z. Geschichte d. Naturwissenschaften u. d. Medizin*, III, 4, 1933, pp. 253 ff.

[8] Cf. L. Edelstein, "The Development of Greek Anatomy," *Bulletin of the Inst. of the History of Medicine*, III, 1935, pp. 241 ff.

discovery of many new data. Dissections and experiments performed by physicians enriched biological and physiological knowledge and made it more precise, as well as more detailed. Yet it is not amiss to recall the fact mentioned before that up until Galen's time not all physicians were interested in inquiries of this kind. And even Galen himself did not believe a study of the entire body to be necessary for medical purposes. Nor did his medical writings impart to physicians more than a description of facts. Their explanation, the scrutiny of the organism as a whole, he put into his philosophical books, which he wrote as a philosopher for philosophers.

Under these circumstances, what Aristotle says of his own time seems indeed true of the entire history of Greek philosophy and of Greek medicine: physicians concerned with biology and physiology—that is, the ingenious and subtle ones, or the more philosophical ones, as Aristotle calls them in distinction to the great mass of doctors—took their departure from philosophy, just as truly systematic natural philosophy ended in a consideration of the principles of life, of health and disease, and of the constituents of the body.[9] Consequently the assumption of an influence of Greek medicine on Greek philosophy must be regarded, I think, as historically incorrect; modern naturalism and ancient naturalism are not identical. To call the Presocratics physicians and scientists rather than philosophers, as has become fashionable, or to say that from Empedocles on "it is impossible . . . to understand the development of philosophy without keeping the development of medicine constantly in view,"[10] is, I am afraid, a misrepresentation of the actual situation. It was as philosophers that the Presocratics embarked on the quest for the nature of things, and trying to detect the causes of all physical phenomena they also dealt with the human body. Nor is it meaningful to assume that Socrates, granted that he was an empiricist, learned the empirical method from medicine. The empiricism of the Hippocratics, as far as it was a conscious method, was itself derived from philosophy, as were their hypotheses concerning the nature of man. If one points to Plato's relation to contemporary physicians, one must at the same time admit that the physicians whom he may have followed were themselves imbued with philosophy. If one holds that biological research culminated in Aristotle's philosophy, one must realize that the concept of the organism which acts

[9] Aristotle, *De Respiratione*, 480 b 26 ff.; *De Sensu*, 436 a 19 ff.

[10] J. Burnet, *Early Greek Philosophy*, 4th ed., London, 1945, p. 201, n. 4.

according to purpose was the discovery of the philosopher Aristotle. The doctors of his generation had no inkling of this and understood the body in terms of those early philosophies which the Platonic *Phaedo* and the Platonic *Laws* tried to combat for the first time. Nor does Stoic philosophy, as outlined in Cicero's *De Natura Deorum,* or Epicurean physiology, as described by Lucretius, indicate a preoccupation with medicine. Like physics, such studies in antiquity were the legitimate concern of philosophers and remained, even when taken over by physicians, philosophical rather than scientific inquiries. While astronomy and mathematics no doubt largely determined the course of ancient philosophy, medicine on the whole stayed within the boundaries laid down by philosophy. In its biological and physiological theories it reflects the history of philosophy, but it does not explain it.

The same dependence of medical investigations on philosophical presuppositions is evident in the rare cases in which a medical treatise touches upon anthropological and ethical problems.[11] The famous Hippocratic book *On Airs, Waters, and Places* derives the human character from climatic conditions. The Asiatics are feeble, not war-like, but gentle, it contends, because in Asia the seasons are uniform and do not change either toward extreme heat, or toward extreme cold. Yet unlike Montesquieu, of whom this claim is reminiscent, the Hippocratic author does not believe that the form of the state also corresponds to natural conditions, such as the size of the country or the climate. To him laws, civic institutions, are an independent creation of the human mind, they have a causality of their own, they themselves create in man a second nature, as it were, so that in a monarchy even naturally brave and spirited people degenerate into cowards. It is Democritus who speaks through the mouth of the Hippocratic writer. Or again, Galen holds that all philosophers and all physicians are unanimous in assuming that the faculties of the soul depend upon the mixture of the elements of the body. Yet he warns his reader not to forget "the creative power of nature which shapes parts according to the traits of the mind," he reminds him that "Aristotle dealing with this very subject wondered whether there was not a beginning more divine, something greater than just heat and

[11] How seldom medical writers dealt with the subject becomes clear from a perusal of the survey given by A. Rivaud, "Recherches sur l'anthropologie grecque," *Revue Anthropologique*, XXI, 1911, pp. 157 ff.; 457 ff.; XXII, 1912, pp. 20 ff. I should mention that physiognomic observations are to be found in Greek medical writings, but they are related to the state of the body rather than to that of the mind.

cold and moist and dry. Wherefore I think it wrong of man to draw such rash conclusions in matters so great and assign to the qualities alone the power of shaping the parts. It is possible that these are nothing more than the instruments and something else the masterhand." [12] Moreover, Galen states, by chosing the right food and drink, by regulating his life correctly, man is able to attain virtue. And he is free to choose what is right; it lies within him to welcome the good, to admit and love it, to shun, to hate, and to flee the evil. To be sure, in Galen's opinion, Chrysippus, the great Stoic philosopher, owed his right understanding of the world to the right mixture of the elements in his body. Nevertheless, he would not detract from Chrysippus' merit; for it was Chrysippus who by building up his body in the right way made himself understand the truth. Unlike Shakespeare, whose lines immortalizing the fatalism of a naturalistic view I have quoted in the beginning, Galen would not allow Nature to lay claim on Brutus' greatness. For the Greek physician agrees with Plato who, though he emphasizes the fact that man's character is shaped by climatic factors, does not on that account dream of absolving him from the responsibility for his actions; or with Aristotle and other philosophers who, much as they stress the importance of natural gifts, of habits and education, do not hesitate to proclaim man's freedom. He expressly denies the validity of the conclusion that the dependence of the faculties of the soul on the faculties of the body makes praise or blame of human actions unjustifiable.

At this point I beg your leave to make a digression. We are prone to quote the maxim *mens sana in corpore sano*, taking it to epitomize the ancients' conviction that only in a sound body can and will there dwell a sound mind. Actually Juvenal, whom we are quoting, says: "Let us pray that there be a sound mind in a sound body." [13] No ancient physician or philosopher, I think, would have believed that a sound body would or could so to say automatically produce a sound intellect and sound emotions. Galen's pronouncement on man's character, to which I have just referred, is, as he himself implies, typical of medical and philosophical thinking. It presupposes the acceptance of the Dogmatic concept of the soul, which through some of its faculties is the organ of

[12] *De Temperamentis*, II, ch. VI [I, pp. 635 f. Kuehn]; I am quoting the translation of the passage given by M. Greenwood, *The Medical Dictator*, London, 1936, p. 33. For the following reference to Galen, cf. *De Sequela*, ch. XI [IV, pp. 814 ff. Kuehn]; cf. also ch. IV [IV, p. 784 Kuehn].

[13] Juvenal, *Sat.*, X, 356: *orandum est ut sit mens sana in corpore sano.*

emotions and mind, while through others it regulates the physical functions. The soul, moreover, or at any rate the mind, is considered a substance different from the body, even if modifiable by it. Philosophers more materialistic than Galen, denying the existence of spiritual substances, assumed at least the autonomy of man. Epicurus more vehemently than Plato or any idealistic philosopher defended human freedom. Stoic fatalism questioned man's freedom of action, not his responsibility for his character. It remained for Descartes to claim that the body was a machine, a mechanism that took care of all bodily movements and also produced emotions. It remained for Spinoza, elaborating on Descartes, to claim that " he, who possesses a body capable of the greatest number of activities, possesses a mind whereof the greatest part is eternal." [14] Descartes saw quite clearly that his concept of the working of the body contradicted the ancient concept of the soul. His new theory was based on Harvey's discovery of the circulation of the blood, from which it seemed to follow that the bodily functions went far beyond anything man had imagined before.[15] Later physicians continuing research along the lines suggested by Harvey, Descartes, and Spinoza, began to uncover the marvels of the body-machine. Medical insight began more and more to change the direction of philosophical thought. Ethics became allied with biology; the mind could now be considered an epiphenomenon of the body.

I am not asking who is right and who is wrong. I have made this digression only in order to accentuate the difference between the ancient and the modern situation. Ancient medicine neither in scientific nor in ethical matters made startling discoveries that heralded a new era; it voluntarily submitted itself to philosophy. This is perhaps one of the reasons for its failure as a science, great as it was as an art. On the other hand, the submission of medicine to philosophy may also in part account for the fact that in all investigations concerning an understanding of the body, as well as of the mind, the leadership rested unchallenged with philosophers.

But it is time for me to dismiss the consideration of these problems and to explain why, in spite of the affinity between medicine and philosophy stressed so far, the two could, as I stated at the very outset, have been opposed to one another in the controversy about the goal of

[14] Spinoza, *Ethics*, V, 39 (transl. by R. H. M. Elwes, *Philosophy of Benedict Spinoza*, p. 274).

[15] *Traité des passions*, I, article 1; 7; cf. *Discours de la méthode*, part V, 7 ff.

life. To make this understandable, I must first of all remind you of the truism that to the ancients, health certainly was one of the highest goods. Indeed, the Sceptic philosopher Sextus does not hesitate to say that to the common people, the men of practical life, health was the *summum bonum* at all times.[16] From post-Homeric centuries onward even poets and writers in ever-increasing numbers contended that without health nothing in this world has any value. To take such an attitude was well in accord with the inborn pessimism of the Greeks, their sceptical doubts as to the chances for happiness, their clear awareness of the tragic aspect of human life. Physicians generally sided with the many, with the pessimists and sceptics. Since the fifth century B. C. they consistently proclaimed that, as Herophilus phrased it, "when health is absent, wisdom cannot reveal itself, art cannot become manifest, strength cannot fight, wealth becomes useless, and intelligence cannot be made use of." [17] And they urged that not only those who were ill, but also those who were healthy should follow the dictates of medicine, in order to remain healthy.

Their demand at first glance may sound reasonable enough. Yet it seems less convincing as soon as one begins to realize its implications. Since health was considered a balance of the various constituents of the human body, at every moment upset by man's actions, by his taking any food or drink, it had at every moment to be restored consciously. Consequently, a healthy person had to watch himself continuously, he had to subject himself to minute rules, he had to guard against any deviation from the prescribed regimen. Only thus could he be healthy and live long, he was told. An odd way of achieving health and longevity! One might well characterize it in the words in which Macaulay describes how oligarchical government achieves its stability, words which in fact are inspired by Plato's description of ancient dietetics. Oligarchical government, Macaulay says, " has a sort of valetudinarian longevity; it lives in the balance of Sanctorius; it takes no exercise; it exposes itself to no accident; it is seized with a hypochondriac alarm at every new sensation; it trembles at every breath; it lets blood for every inflammation; and thus, without ever enjoying a day of health or pleasure, drags out its existence to a doting and debilitated old age." [18]

[16] Sextus Empiricus, *Adv. Mathem.*, XI, 49.

[17] Herophilus *apud* Sext. Empiricum, *Adv. Mathem.*, XI, 50. For details of the medical theories here referred to, cf. L. Edelstein, "Antike Diätetik," *Die Antike*, VII, 1931, pp. 255 ff.

[18] I am quoting Macaulay's remark from P. Shorey's note on the *Republic*, 406 b [Loeb].

But obeisance to such an ideal does not merely stifle enjoyment and pleasure; it makes man unfit for life. It makes him the slave of his body. He loses all freedom of action and decision; he cannot fulfill his duties as a citizen. Plato was not slow in pointing out all these dangers, nor did the Romans overlook them; their initial resistance to Greek medicine was to a large measure due to their opposition to doctrines which threatened to destroy normal political and civic life. Besides, such a regimen of precaution and concern with the body, as again Plato reproachfully remarked, can be followed by the wealthy alone, by those who are not forced to earn a living. Physicians did not deny the correctness of this charge, but they dismissed the case of the poor as beyond repair and turned all their attention to the rich, the only ones who in their opinion could live a worth-while life. And there is little doubt that many among the great and the wealthy heeded the advice of the physicians, especially in those centuries in which the city state began to lose in importance, or in which the political activity of the individual was diminishing, that is, in the Hellenistic period and in the time of the Roman emperors. Desiring health as they did, unhampered by any professional obligation, uninterested in being busy for the sake of being busy, the upper classes were free to live for their health, to indulge in the idolatry of the body endorsed by the physicians.

It was inevitable therefore that philosophers passionately fought against medicine and its glorification of health, just as they had opposed the agonistic ideal and the glorification of the athlete. More consequential even than their criticism of the political and social dangers which the teaching of medical dietetics promoted was their vigorous denial that health was the *summum bonum*, that it was identical with happiness, the aim toward which all human endeavor should be directed. Naturally, it is not my intention to contend that Plato, Aristotle, the Cynics, the Stoics, the Epicureans were altogether contemptuous of health. Before the slow disintegration of ancient culture set in, no Greek would have gone to that extreme; health remained a good to be at least preferred, as the Stoics put it. But philosophers were unanimous in their judgment that values other than health are superior. To give more emphasis to their point of view they even laid down their own rules for a proper regimen. What Plato in the *Republic* and in the *Laws* says about gymnastics, the education of children, the daily routine of men and women, what Plutarch sets forth in his charming little book *Advice on How to Keep Well*, serves to teach all and sundry how to live healthily without

putting oneself into a strait jacket and without neglecting one's social and moral obligations.

To repress the dangers threatening from the leadership of physicians in society, Samuel Butler in his *Erewhon* made it a crime to be sick and therefore to be in need of medical care. Otherwise, the judge in the Erewhonian trial says, "the doctors should be the only depositaries of power in the nation, and have all that we hold precious at their mercy. A time of universal dephysicalization would ensue." [19] Butler's solution of the difficulty as he saw it may be ingenious, but it is hardly practicable. The problem was solved more reasonably and adroitly by Greek philosophers. Even Plato did not go beyond restricting the physician's practice to cases of emergency. However, he and Aristotle and all subsequent philosophers raised the question of who should determine what is valuable and what is not. The physician decides whether his patient ought to take walking exercise or not; he does not and cannot tell him whether he ought to be healthy or not, whether his life should be saved at all cost. In other words, the positing of aims lies with philosophy, not with the art of medicine, or with any other art or science, for that matter.[20] The physician therefore must mind his own business and not interfere with things that are beyond his reach. He may excel in preserving the health of those who wish to be healthy, in healing the diseases of those who wish to get well; but he cannot tell men what they should wish. In modern times, the physician has become the confidant of the healthy and of the sick, the expert of social agencies, the counselor of the judge, the authority to whom even the planners of foreign and domestic policies look up. To the Greek philosophers, he was no advisor on morals, justice, or politics. To them, medicine was not another form of education or philosophy. They clung to their prerogatives and insisted on their absolute sovereignty.[21]

Nevertheless—and this brings up the last aspect of my subject which I wish to discuss—whenever philosophers tried to interpret for the benefit of others the significance and meaning of their own endeavors, they could not find any parallel more illuminating than that of philosophy and

[19] Ch. XI (Some Erewhonian Trials).

[20] Cf. *Ethica Eudemia*, II, 2, 1227 b 25 ff.; cf. also *Ethica Nicomachea*, III, 3, 1112 b 12-16; *Magna Moralia*, I, 1, 1182 b 28-30. Cf. also Plato, *Laches*, 195 c-d.

[21] The dissension between physicians and philosophers is not taken into account by W. Jaeger, *Paideia: The Ideals of Greek Culture*, New York, III, 1944, pp. 3 ff.; this is one of the main reasons why I cannot agree with his evaluation of the role of medicine, cf. especially pp. 44 f.

medicine. Writers of all philosophical creeds gave a prominent place to the analogy of body and soul, to the similarity between the training of the body and the discipline of the soul, to the consideration of medicine as a counterpart of ethics, and, as I suggested before, it was perhaps in the usefulness of this comparison for the philosophical argument that lay the greatest debt of philosophy to medicine. What were the reasons for this peculiar predilection which philosophers showed for the medical simile? The frequency of references to medicine in philosophical books has, of course, often been noticed, but so far it has not found a satisfactory explanation.

Now, the starting point of all the comparisons between body and soul is perhaps not too difficult to understand. The philosopher, insisting on the value of the soul and proclaiming its superiority over the body, was faced with man's natural partiality to his body. He was therefore driven to contrast the two; he had to try to turn man's thought from his body and its interests to his soul and its concerns. In this attempt the Greek philosopher, while on the one hand finding his task especially difficult due to the Greeks' love of the body, gained on the other hand a specific advantage. From the fifth century B. C., that is, from the time when the discussion of ethical problems became an integral part of philosophy, medicine propounded the doctrine that the body needs conscientious care if it is to perform its functions properly. Philosophy therefore found in medicine a basis for exhortation which appealed strongly to the Greeks. Was it not obvious to conclude that what is true of the body must also be true of the soul, since body and soul are both parts of our being? That in fact it must be true to an even higher degree in regard to the soul which is so infinitely more precious?

Yet the protreptic appeal to the body and its medical needs, intended to enforce the dogma that the soul likewise needs care, was not merely emotional, it implied at the same time another purpose. In the beginning of the *Nicomachean Ethics*, Aristotle parallels his teaching concerning moral qualities with the teaching concerning health, because, he says, it is necessary to explain what is invisible by means of visible illustration.[22] The soul is invisible; knowledge about it is elusive. By comparing the soul to the body, that which is seemingly unreal is translated into comprehensible language. And if one remembers that in antiquity everybody was familiar with medicine—it was the only art or science about

[22] II, 2, 1104 a 13-14.

which everybody knew something; physicians wrote books for the general public; laymen discussed medical problems with their physicians; in short, medical knowledge was perhaps diffused more widely in Greek and Roman times than in any other period of history—one immediately realizes the particular forcefulness of such medical illustrations. In referring to medical insight the philosopher speaks of something that people can be expected to possess and that will help them to grasp the new kind of insight that they are expected to acquire through philosophy.

At this point, however, you may ask: how can medical knowledge, knowledge of the body, be helpful toward an adequate understanding of the soul? Are not body and soul essentially different? Is it not just the difference between the two which the philosopher tries to point out? My answer is that it is characteristic of Greek ethics that it conceives of the soul in the likeness of the body, that it visualizes the soul in very much the same way in which the body was understood, not on a highly technical level, to be sure, but rather in terms of that medical doctrine which was basic and almost universally accepted. The body has its desires, it strives to be filled, to be satiated. These bodily appetites in themselves tend to be extreme; they have no self-restricting limits. In order to prevent disease, to attain health, it is necessary to check the natural tendencies, to introduce measure, a standard by which to curtail the unlimited appetites. The desires of the soul, in the view of the Pythagoreans, of Plato, and of Aristotle, likewise tend to extremes; they strive unceasingly to be satisfied. Pythagorean ethics, Platonic justice, the Aristotelian doctrine of the mean, teach the measure, the standard by which the extremes of our passions are to be reduced to their right proportion. Nor did the body fail to provide an example when Cynics, Stoics, and Epicureans considered all natural activities of the soul, all natural emotions as bad, or even diseased. Does not language itself indicate the similarity of body and soul? Are not the emotions called by the same name, *pathe*, that denotes the sickness of the body? Is it not true—to use Lucretius' words—that "our soul itself has a flaw and that by this flaw an inward corruption taints all that comes from without, even though it were a blessing"?[28] And is it not equally true that the body and its natural functions are diseased? If man is not constantly on the lookout, he continually falls into disease, he is continually sick. This was exactly the theory on the basis of which the dietetics for the healthy

[28] *De Rerum Natura*, VI, 17-19 [Loeb translation, adapted].

was developed, as I have pointed out before. Like the body then, which must be made healthy, so the soul, sick by nature, needs purging, as Lucretius says, or treatment, as the Stoics contend, in order to be healthy.[24] Only in this manner can virtue be achieved, that state of eudaemonia which philosophers never tired of celebrating as "the health of the soul."

Important as the medical simile proved to be in all these respects, its deepest significance becomes apparent only in the consequences drawn from the analogies I have mentioned so far. To understand their full bearing it is necessary to recall one feature of Greek medicine that was obvious to the ancients, yet is rarely appreciated by modern interpreters. Greek medicine reversed man's natural beliefs; it had something paradoxical about it. It seems natural to suppose that we are healthy as long as we are not sick, that if we follow the instincts of our body we will be well. The teaching of Greek dietetics, the doctrine according to which every one, even the healthy person, must care for his body, ran counter to man's natural feelings. Moreover, it always remained strange to the ancients that man should renounce his rights over himself and should obey the physician, who reigns over him as the king reigns over his subjects, to borrow a phrase of Galen.[25] To accept such a rule again seemed unnatural, for the body is our own; we are its masters, and no one else. The irritation felt was aggravated by the fact that the free citizen, the rich man, had to follow the commands of a craftsman who often was a slave. Finally, the Greeks and Romans never ceased wondering about the paradox that by cauterizing and cutting, by doing violence to his body, man should achieve health, that good should come of something that in itself is painful and causes suffering.

It was precisely this paradoxical truth that reason and experience had taught man to accept in medical matters, on which philosophers seized for their purposes. You find the philosophers' teaching paradoxical, Epictetus asks. And he answers his own question by saying: "But are there not paradoxes in the other arts? And what is more paradoxical than to lance a man in the eye in order that he may see? If anyone said this to a man who was inexperienced in the art of surgery, would he not laugh at the speaker? What is there to be surprised at, then, if in philosophy also many things which are true appear paradoxical to the inexperi-

[24] *De Rerum Natura*, VI, 24; Chrysippus, Fr. 471 Arnim.
[25] *De Methodo Medendi*, I, 1 [X, p. 4 Kuehn].

enced?" [26] Indeed the whole range of the transvaluation of values that was brought about by philosophical ethics, could best be illustrated by the medical example.

The paradoxical quality of Cynic and Stoic teaching concerning natural feelings and emotions needs no emphasis. Their notion of the "health of the soul" was as shocking, the very antithesis of the natural, as was the medical teaching about the health of the body. Nor did Lucretius deny that Epicurus' doctrine of "the healthy soul" seemed "somewhat harsh to those who have not used it, and the people shrink back from it." [27] By pleasure they understood something quite different from that which Epicurus considered pleasure. But did Plato's philosophy contribute less toward changing common beliefs? To be sure, he still upheld the old values; but did he understand them in the same way? Is it natural to believe that it is better for man to suffer injustice than to do injustice, if he must choose between the two? Plato found it necessary to turn man's neck, so as to make him see the light of truth.[28] Or is Aristotle's ethical theory opposed to a lesser degree to man's instinctive feelings? Can one immediately understand his contention that matters of conduct and expediency have nothing fixed or invariable about them, that moral qualities are so constituted as to be destroyed by excess no less than by deficiency? [29] Aristotle at any rate did not think this to be self-evident and took great pains to prove it.

Moreover, if man must acquire the knowledge needed to regulate his physical life from the physician, the expert, who has found it, if he must submit himself to his judgment, to his guidance, the same demand was made by the philosopher, and again it was upheld regardless of the social position of the expert, of the ruler, and of the ruled. It is true, Plato still believed that the individual by himself should and could find the truth. Socrates, to him, was not a physician; his help in finding the right answer was comparable to the assistance given by the midwife; man's knowledge is his own child, as it were. But wherever Plato tries to solve ethical questions within the framework of political theory, he finds no more cogent parallel to the position which the true statesman ought to occupy than that of the physician. Statesmen must rule, he says, "whether they are rich or poor, over willing and unwilling subjects alike, with or

[26] Epictetus, *Dissertationes*, I, 25, 32-33 [Loeb].
[27] *De Rerum Natura*, IV, 18-20 [Loeb].
[28] *Republic*, VII, 515 c.
[29] *Ethica Nicomachea*, II, 2, 1104 a 11 ff.

without written laws, just as physicians must prescribe cures with or without the patient's consent, and whether they do so on the basis of written laws or without them, and whatever they do, whoever they are, we call them physicians." [30] Aristotle, on the other hand, bids us to be our own doctors, hitting the right mark through our own insight. With the Cynics and Stoics the philosopher himself became a doctor, the expert who diagnosed the disease of man. For the diagnosis of our passions, says Galen, cannot be entrusted to ourselves; it must be entrusted to other people, and " it is not just any chance person to whom this oversight can be given." [31] Ethics thus became *medicina mentis*, medicine of the soul; the philosopher came to rule over his client with absolute power.

Nor was the final point of comparison overlooked: the treatment cannot be soft and in accordance with our wishes; it must be painful, it must hurt. Plato insisted that the true statesman must be allowed to kill or to banish people, just as the physician is allowed to heal by cutting us, or burning us, or by causing us pain in any other way.[32] And while Lucretius was intent on lessening the bitterness of the remedy by adding " the delicious honey of the Muses," [33] the Stoics delighted in emphasizing the harshness of the treatment. It may suffice to quote Epictetus: " Men, the lecture room of the philosopher is a hospital; you ought not to walk out of it in pleasure, but in pain. For you are not well when you come; one man has a dislocated shoulder, another an abscess, another a fistula, another a headache. And then am I to sit down and recite to you dainty little notions and clever little mottoes, so that you will go out with words of praise on your lips, one man carrying away his shoulder just as it was when he came in, another his head in the same state, another his fistula, another his abscess? And so it's for this, is it, that young men are to travel from home, and leave their parents, their friends, their relatives, and their bit of property, merely to cry 'Bravo!' as you recite your clever little mottoes? Was this what Socrates used to do, or Zeno, or Cleanthes? " [34] It is indeed true that this paradox, like all others, could best be made clear by the paradox of medicine, as Epictetus says.

[30] *Politicus*, 293 a-b. For Plato and the simile of medicine, cf. also L. Edelstein, " The Role of Eryximachus in Plato's Symposium," *Transactions of the Amer. Philol. Assoc.*, LXXVI, 1945, pp. 98 ff.

[31] *De Animae Perturbationibus*, I, ch. vi [V, p. 30 Kuehn] transl. by A. J. Brock, *Greek Medicine* [The Library of Greek Thought], 1929, p. 170.

[32] *Politicus*, 293 b.

[33] *De Rerum Natura*, IV, 22.

[34] *Dissertationes*, III, 23, 30-32 [Loeb].

I have come to the end of my analysis of the relation between Greek philosophy and medicine. Many intricate questions I could only touch upon. Many difficult problems I could discuss only in a general way. Details have still to be worked out. The characteristic differences prevailing in the various centuries must be marked more clearly. But this much, I think, can be stated safely: medicine did not influence philosophy by giving to it scientific information, nor did it help philosophy to find a solution of ethical questions. But medicine did serve philosophy as a means of explaining and of making acceptable to men that conclusion which philosophy itself had reached, that man can live without philosophy as little as he can live without medicine.

VILLERMÉ AND QUETELET

ERWIN H. ACKERKNECHT

Among the papers of A. Quetelet a lot of 86 letters of the French hygienist, Louis-René Villermé, written between 1826 and 1863, was found.[1] The writer as well as the recipient of these letters were men of sufficient importance [2] to justify an analysis. In addition it must be stated that very few other personal documents on Villermé are known. This is no accident. Villermé was an introvert, "a man who [according to himself] liked only a few people"; and a man who, as he called it, was "too lazy" to write, and who would rather travel 20 miles to see a friend than write a letter. ("Je suis de tous les animaux écrivains le plus paresseux." Yet Villermé was not "too lazy" to write numerous scientific papers.) Quetelet was one of the few exceptions Villermé made during his long life as to personal intimacy and letter writing. The last letter Villermé ever wrote was, according to his daughter, Madame de Fréville, directed to Quetelet. Even thus Villermé wrote only from one to four letters per year to his friend—it is true letters were not their only means of communication; both being rather enthusiastic travelers, they visited with each other at intervals of about four to five years. It took Villermé seven years to pass from the formal address "Monsieur" to "Monsieur et ami," and only after 1834 was Quetelet addressed as "mon cher ami."

The correspondence starts in April, 1826, that is, about two years after the fateful trip of the 27 year old Lambert Adolphe Jacques Quetelet to Paris. Quetelet had been teaching mathematics in Ghent and Brussels since 1817. At the end of 1823 he was sent by the Belgian Academy to Paris in order to familiarize himself with astronomical apparatus in view of the erection of an observatory in Brussels. He was named astronomer

[1] I owe a microfilm of these letters to the thoughtfulness of Dr. Jean Pelseneer, who drew my attention to them, to the kindness of Mr. Georges Quetelet, grandson of the great statistician, and the generosity of the Belgian Committee for the History of Science. On a study trip to Paris in the summer of 1951, supported by the American Philosophical Society, I was able to collect additional material relevant to this correspondence.

[2] As to the importance of Villermé and the French hygiene movement he was part of, see Ackerknecht, E. H., Hygiene in France, 1815-1848, *Bull. Hist. Med.* 22: 117 ff., 1948. The only (very sketchy) monograph on Villermé is the 1939 Paris M. D. thesis of Marcel Delabrosse. Most of my data on Quetelet's life are taken from Hankins, F. H., *Quetelet as a Statistician*, New York, 1908. See also M. Halbwachs on Quetelet in the *Encyclopedia of Social Sciences* for more literature.

in 1828 but could occupy the observatory only in 1832. His connection with the Academy remained close; he was its director in 1832-33 and its perpetual secretary from 1834 to 1874, that is, the time of his death.

During the months which Quetelet spent in Paris in 1823-24 he befriended the astronomer Bouvard and through Bouvard the mathematicians, Laplace, Poisson, Fresnel, and Fourier. It is quite obvious that the influence of these men, all deeply interested in the calculus of probability and in statistics, decided Quetelet's further evolution. Outstanding as his former mathematical and later astronomical and meteorological work remain, he is remembered primarily as the inventor of "l'homme moyen" and "the central figure" in "the era of enthusiasm (1830-48)" in the history of statistics.[3] According to M. Georges Quetelet, L. A. J. Quetelet's acquaintance with Villermé dated from 1825, and was due to the initiative of Fourier.

Quetelet collaborated in census work with Edouard Smits (1789-1852) since 1826. In 1841, after Smit's retirement, he inspired the creation of a "Commission Centrale de Statistique" in Belgium which was imitated in many countries and which he directed very successfully up to 1874. His census of 1846 is supposed to have opened a new era in census taking. Belgian delegate at the meeting of the British Association for the Advancement of Science in 1833, he suggested the formation of the statistical section of the Association. He became the organizer of the International Statistical Congresses, the first of which was held in Brussels in 1853. which played a tremendous role in the growth and standardization of statistics. Quetelet published continuously on the subject since 1825, and his influence on the field cannot be overestimated.

Villermé, born in 1782, thus 14 years older than his friend, had, when the correspondence began, already finished a whole period of his life. For ten years (1804-1814) he had followed Napoleon's armies all over Europe. He was an able surgeon, publishing in 1815 the amputation procedure that is usually named after Lisfranc.[4] After his discharge from the army and especially after his marriage in 1818 he rather rapidly shifted away from medical practice to writing. Even his writing soon concentrated more and more on public hygiene and its statistical implications, a field in which he started with a memoir on prisons in 1820, and in which he was to become famous. His writings filled an urgent need as

[3] Westergaard, Harold, *Contributions to the History of Statistics*, London, 1932, p. 166 ff.
[4] Archives Générales de Médecine, 2: 535, 1823.

epidemiology was, according to Raige Delorme, then equally neglected by the two predominant schools, the " nosological " and the " physiological." [5] In the Société Médicale d'Émulation of which he was secretary in 1822, he presented mostly clinical material. In the Academy of Medicine into which he was elected in 1823 together with Rayer and Andral, his communications dealt from the start with hygiene and statistics. He also was a cofounder and active editor of the *Annales d'Hygiène Publique et de Médecine Légale* in 1829. In 1832 he was called into the Académie des Sciences Morales et Politiques when this body was resuscitated 30 years after Napoleon had suppressed it on account of its " ideologist " members. For this Academy he made research trips in 1835, 1836, 1837, 1840, and 1846 to study the condition of the working classes in France and neighboring countries, studies that led to his famous " Tableau " in 1840. Villermé was also a member of the Conseil de Salubrité, 1831-1836, and of the Conseil Supérieur d'Hygiène in 1848. When he died, in 1863, at the age of 81, he was still working. He left unfinished a statistical memoir on the respective ages of spouses.

The basic approach and the fields of interest of Villermé and Quetelet were very similar, and as Villermé was older, he had on many questions preceded Quetelet, and Quetelet benefited greatly for his great synthesis from Villermé's detailed work and advice. Quetelet was always the first to underline to what extent he was indebted to Villermé. In the last version of his *Physique Sociale* (Brussels, 1869) he quotes him no less than 48 times; this is more than twice as much as the next often quoted authors: Sadler (21 times); Casper (19); Poisson (19); Laplace (18) and Benoiston de Chateauneuf (18).

The letters of Villermé strengthen this impression. We encounter critical remarks by Villermé which we find later without special reference in Quetelet's work. Thus Villermé wrote, for instance, on April 25, 1829:

> Vous pensez que c'est dans l'évaluation fautive de la population de vos provinces qu'il faut *plus particulièrement* chercher la cause des différences que ces provinces présentent sous le rapport de la proportion des naissances et des décès. Je ne saurais avoir une opinion sur l'évaluation dont il s'agit. Mais ce que je soutiens c'est qu'elle n'est pas aussi fautive que vous paraissez de croire, au moins pour la Zélande qui doit produire, proportion gardée, plus d'enfans qu'aucune autre province du royaume, parce que l'insalubrité y est très grande et par contre la mortalité très forte. La proportion des naissances est toujours réglée par celle des déces, en d'autres termes,

[5] *Dictionarie de Médecine*, ed. by Adelon, Beclard, etc., Paris, 1832, vol. I, p. XIX.

c'est la consommation des hommes qui determine leur production, du moins dans nos regions populeuses de l'Europe.[6]

The same idea is found in Quetelet's *Physique Sociale* in volume I p. 286 (1869 ed.). The same holds good for Villermé's ideas on the value of height studies, on the results of juries depending on probability, etc. Quetelet did, on the other hand, by no means always follow Villermé's critical advice. Thus, for instance, he took over into his *Physique Sociale* his " correlations " between race and crime in spite of Villermé's criticism of these ideas when they first appeared in a memoir of Quetelet.

Both men were accused of materialism in their early days.

Je vois par votre lettre que je ne suis pas le seul que mes études font accuser de matérialisme. La Gazette de France, le Nouveau Conservateur, et, ne l'a-t-on dit, la Quotidienne aussi, m'ont prêté des impertinences que je n'ai point dites afin d'avoir occasion de crier contre moi. En vérité, il faut être furieusement enragé pour vouloir nous mordre. Puis qu'on vous met dans la meme compagnie que moi, je m'en consolerais tout de suite, si j'avais pu m'affliger plus d'un quart d'heure d'une pareille attaque. (April 25, 1829)

Forty years later Quetelet found it necessary to emphasize strongly his belief in God.[7] And Villermé was, according to Guerard's necrology in the *Annales d'Hygiène Publique* (2. s. 21 : 162 ff, 1863), in close relation with the famous Father de Ravignan. The fact that the letters do not contain any allusions or even expressions related to such a reorientation of Villermé makes Guerard's information appear less weighty.

The work of both men is reflected in Villermé's letters. There are mentioned, in 1827 and 1828, Quetelet's memoirs on the population of the Netherlands; in 1831 and 1832 the basic ones on human growth and crime; research on reproduction and mortality, together with Smits, in 1832; the famous memoir on relations between height and weight in 1833; the statistical yearbooks of 1834, 1838, 1843; memoirs on mortality in different seasons (1834); marriage and free will (1847); mortality tables (1853).

Quetelet summarized most of his findings and ideas in his *Essai sur le Dévelopment des Facultés de l'Homme* or *Physique Sociale* (1835). As Villermé saw the first edition of the book through the press it is often mentioned in the letters. Villermé formulated his opinion on it as follows:

[6] Here as elsewhere the original spelling of the letters has been retained.
[7] Phys. Soc. ed. 1869, vol. I, p. V.

Quand à votre ouvrage, depuis que j'ai lu la derniere partie, je n'en pense pas plus mal qu'auparavent. Sans doute, il ne me satisfait pas sur tous les points et je ne saurais être toujours bien d'accord avec vous. Mais à vos documents viendront s'enjoindre d'autres, et telle proposition qui me semble encore douteuse deviendra certaine. C'est l'affaire du temps, sachons attendre. Vos deux volumes seront le point de départ d'une foule de recherches importantes, et seront cité un jour comme on cite l'ouvrage de Pierre Sussmilch. Dans dix ans, ils seront beaucoup plus lus et beaucoup mieux compris qu'aujourdhui. (August 31, 1835)

Villermé repeated the two latter affirmations April 30, 1837, and concluded " Je suis fier de pouvoir dire que j'ai l'amitié de l'auteur." Villermé was far more critical of Quetelet's *Lettres à S. A. le Duc Régnant de Saxe-Cobourg et Gotha sur la Théorie des Probabilités Appliqués aux Sciences Morales et Politiques.* (Brussels, 1846).

La forme épistolaire que vous avez donnée à votre travail me semble ne convenir ni aux savans ni aux autres; les Entretiens de Fontenelle sur la pluralité des mondes trouveraient même bien difficilement aujourdhui beaucoup des lecteurs; du moins chez nous. Il fallait choisir le genre de Fontenelle ou bien celui de Laplace, et à force d'artifice se montrer supérieur à l'un et a l'autre. Un tel sujet, la theorie des probabilités, se concilie mal d'ailleurs avec le mot d'altesse et avec (illisible) de plusieurs de vos lettres. Dans ma pensée votre livre n'aura donc point la réputation de votre Physique Sociale à laquelle il doit faire suite. Ce jugement restera entre vous et moi et je vous aime assez pour désirer me tromper. (February 18, 1846)

This judgment remained so much between two friends that under some pressure of Quetelet, Villermé gave a favorable report on the book in the Academy. Quetelet's next book, *Des Systèmes Sociales*, also seen through the press by Villermé, suffered much from coming out just in the midst of a revolution. Quetelet abandoned here the epistolar form criticized by Villermé.

Villermé referred continuously to his own writings. As a matter of fact several works of his are listed in these letters that are not mentioned in the so far existing, very defective bibliographies. In 1826 he sent to Quetelet a memoir on population movement in Paris and the one on the statistical problems of life expectancy. He also did work on mortality in the " Dépots de mendicité " and the Maternité Hospital. Also in 1826 he discussed the connection between births and season, births, and periods of religious fasting, etc. In 1829 he gave his only course of lectures (on population and hygiene) and published on mortality in prisons. The cholera epidemic in 1832 brought about several publications of Villermé who was very active during the epidemic. We will report later on his

very long letter on the subject of June 25, 1832, when discussing anti-contagionism.

In 1832 he analyzed the sex ratio in legitimate and illegitimate births, in 1833 the economic and statistical implications of epidemics, in 1834 the deleterious influence of swamps, especially in regard to child mortality.

In 1835 he started the numerous "field trips" in order to explore for the Academy the moral and physical situation of textile workers. Trips and publications on this subject would continue, even long after Villermé had published his *Tableau de l'Etat Physique et Moral des Ouvriers Employés dans les Manufactures de Coton, de Laine, et de Soie.* (2 volumes, Paris, 1840). Villermé regarded this as his most important work, and it has indeed kept his name alive. He felt very strongly about the horrors of child labor that he had uncovered, and a French law of 1841 against child labor was mostly due to his efforts. Back from his first exploratory trip, Villermé reported (under the 9th of June, 1835) to his friend:

> Vous me demandez si je suis satisfait de mon voyage. Oui, sous certains rapports; non sous d'autres. J'ai été a Lyon, Avignon, Nismes, Montpellier, Marseille, Toulon, Geneve, Lausanne, Berne, Zurich, Basle, Mulhouse, et dans les environs. J'ai partout rencontré des gens qui m'ont accueilli et ont favorisés mes recherches. Si par suite de mon voyage, une loi fixant, pour les enfans le ***maximum*** de la durée de travail dans les manufactures, pouvait être adoptée, je serais pleinement dedommagé de toutes mes peines. Cette loi qui ne serait que l'imitation d'une autre rendue depuis peu en Angleterre est impérieusement réclamée par la morale et l'humanité. Car dans plusieurs de nos manufactures on exploite la détresse des petits enfans, non pas, je crois autant qu'on le faisait naguère de l'autre coté de la Manche, mais de manière cependant à miner leur santé. Imaginez vous que dans plusieurs établissemens on tient pendant 16 heures, pas moins, de malheureux enfans, qui n'ont pas encore 8 ans ou les ont à peine, et c'est à peine qu'on leur accorde 2 heures 1/2 pour les repas et le repos.
>
> On concoit très bien que les chefs des fabriques analogues qui voudraient limiter la durée du travail quotidien à 10 heures pour les jeunes enfans, ne pourraient pas soutenir la concurrence contre les autres, sans diminuer le salaire, et que dans ce cas les parents aimeraient mieux placer leurs enfans dans les ateliers où la journée est plus forte.

In his next letter of August 31, 1835, Villermé returns to the question of child labor, hoping that a French law will be without the loopholes of the English. In a letter of June 6, 1837, Villermé sends a memoir on child labor to Quetelet urging him to submit this question to the Academy, even to the press. "Il ne s'agit pas de moi, mais d'une question

d'humanité, et il faut former l'opinion sur elle." On January 13, 1841, he again hopes that Belgium will follow France with a new law against child labor. On March 9th, 1843, he shows why the law did not work in France:

On annonce que vos Chambres vont s'occuper d'une loi sur le travail des enfans dans les manufactures. La nôtre serait excellente si elle obligeait le gouvernement à rendre compte tous les ans aux chambres de son exécution. L'oubli de cet article est cause de la non nomination jusqu'ici d'inspecteurs réels qui veillent a l'exécution de cette loi, et cette non nomination est à son tour cause de la nonexécution dont il s'agit. It arrive avec nos inspecteurs beaux honoraires, ce qui est arrivé avec les inspecteurs non retribués de l'instruction primaire. Au bout d'un an, le zèle était rafroidi, et les inspecteurs ne prenaient ce titre que sur leurs cartes de visites.

Je vous engage bien à profiter de votre influence sur les personnes de votre gouvernement pour faire adopter la disposition dont je parle. Si je croyais pouvoir y contribuer personnellement j'irais à Bruxelles quelques jours avant la discussion de la loi.

The letters show very graphically the tremendous amount of travel and work involved in the collection of Villermé's material on the situation of the industrial worker.

La vie que je mène en voyage est prodigieusement active; toujours debout, toujours dans les atéliers, avec les ouvriers, chez eux ou m'occupant d'eux; je prends à peine le temps de recueillir des notes, et c'est ici qui je rédige. (January 1, 1836)

Villermé's work on the mortality of different professions was, of course, closely related to his labor survey. In 1845 he compared statistics collected in Belgium under Quetelet's direction to those collected in France under Moreau de Jonès, much to the detriment of the latter. In 1849 he commented on his pamphlet against production cooperatives (associations) which appeared in a series of anti-revolutionary propaganda pamphlets launched by the Academy with the collaboration of Cousin, Troplong, Passy, Charles Dupin, Mignet, Barthelmy, St. Hilaire, Thiers and Portalès. In the same year Villermé examined the problem of governmental life insurance for workers; in 1850 the problem of customs and smugglers, and in 1860, the respective age of spouses.

Villermé was a sincere admirer of Quetelet's work and praised it again and again publicly and in his letters. He encouraged the younger man in reporting to him favorable comments of others on his work. If, on the other hand, he disagreed, he said so with the straightforwardness and honesty for which he was known among his contemporaries. He was

essentially a very humble man. On April 25, 1829, he wrote, after 10 years of successful work in the field, to Quetelet, rather a newcomer:

> Je suis très content que ma manière de considérer la statistiques s'accorde entièrement avec la votre; la lère note de l'ouvrage que vous m'avez envoyé, en est la preuve. Au reste, c'est dans les écrits de ceux qui cultivent cette science, et je crois aussi dans les votres que j'ai pris mes idées et peutêtre même les expressions dont je me suis servi.

Out of admiration, sympathy, and humility Villermé rendered innumerable, time-consuming, and unrewarding services to Quetelet. As a matter of fact, the bulk of the letters consist of such technical details as: I went today again to see your publisher (Villermé saw two books of Quetelet's through the press); I enclose excerpts from British and Italian statistics; I sent you today the books you asked for; I reported for this Academy (or journal) on your last paper; I distributed your reprints; this would be a good subscriber for your correspondence, etc., etc.

On a somewhat higher level the services rendered were more mutual. When Villermé made Quetelet a corresponding member of "his" two Academies, that of medicine and that of moral sciences, Quetelet had already succeeded in making him a member of the Belgian Academy. Quetelet's *Correspondance Mathématique* and *Annales de l'Observatoire Royal de Bruxelles* reported not less faithfully on Villermé's accomplishments than did Villermé's *Annales d'Hygiène Publique* on those of Quetelet.

The letters show us a rather close knit little world of friends and colleagues: the budding world of statistics, cosmopolitan in its composition, not free from strife and competition, yet, in general, rather touching in its eagerness and solidarity. Villermé refers often to a Belgian group comprising besides Quetelet, the Baron Keversberg de Kessel (1768-1843); Edouard Smits (1789-1852), statistican and poet; Ed. Ducpétiaux (1804-68), lawyer, politician and prison reformer; and Dr. Guiette.

In Paris we hear much about the astronomer Bouvard (1767-1843) and Villermé's close friend, the statistician Benoiston de Chateauneuf (1776-1856) who had also been an army doctor up to 1810. A common friend was also the "moral philosopher" F. X. J. Droz (1773-1850); other members of this statistical movement seem to have been Villot; the famous psychiatrist Esquirol (1772-1840); W. F. Edwards (1777-1849); I. J. Bienaimé (1796-1878). Villermé greatly mourned the disappearance of the economist J. B. Say (1767-1832). "L'Économie

Politique n'a plus en France de représentant digne d'elle." Although Quetelet was involved later in priority discussions with the statistician Guerry de Champneuf (1802-66), Villermé managed to remain on friendly terms with him. Inevitably there was considerable business to transact with the (inefficient) secretary of the Academy of moral sciences, Charles Comte (1782-1837), lawyer and politician, and with his successor, the historian F. A. M. Mignet (1796-1884). Villermé's pet enemy of the period, César-Moreau, is by now so utterly forgotten that he can't even be found in the routine biographic directories.

Two Italian exiles were often mentioned: one, the geographer Balbi (1782-1848) residing in Paris; the other G. Arrivabene (1787-1881) living in Brussels. Villermé always showed particular interest in things Italian and especially Sardinian; this was partly due to the fact that his wife's family came from Lombardy.

In Geneva our friends were apparently closest to Sir François D'Ivernois (1757-1842) who had been an exile in England from 1792 to 1814, and to H. C. Lombard (1805-1895).

At the formation of the Academy the possibility of membership of Malthus (1766-1834) was seriously questioned although to Villermé he was " Certes le plus éminent dans la science." " Vous ne vous figurez pas les préventions qui existent contre lui; c'est un homme qui prêche la dépopulation et le libertinage effrené, qui voudrait qu'on jetât la moitié des petits enfans dans les rivières." While Quetelet had numerous personal contacts in England, we do not hear of any from Villermé. In Berlin Villermé knew well Casper (1796-1864), Dieterici (1790-1859), and Alexander von Humboldt (1769-1859); in Copenhagen C. G. N. David (1793-1874). The friends discussed, of course, the organizational aspects of the statistical world, the Paris statistical society, the International Congresses, etc.

G. Gluge (1812-98) a pupil of Johannes Mueller, was introduced to Quetelet, with whom he was to collaborate closely as is obvious from later editions of the *Physique Sociale*, by the following letter of September 14, 1837.

Mon cher ami,

Mon confrère Magendie, l'un des hommes les moins complaisants du monde en fait de recommendations et d'éloge, m'adresse Mr. le docteur Gluge qui désire se fixer à Bruxelles. Magendie me prie de lui donner une lettre d'introduction auprès de quelque personne honorable de votre ville. Comme je n'en connais de plus honorable que vous, je vous l'adresse à mon tour. Voici comment Magendie termine sa lettre:

" Mr. Gluge, auteur de plusieurs travaux intéressans, est certainement un de nos jeunes travailleurs les plus instruits et les plus capables."

À ce titre il ne pourra manquer d'être bien avec vous, et je n'aurais fait que hâter ce qui aurait toujours lieu par la force des choses.

tout à vous
Villermé

Almost the only place in the correspondence where Villermé discusses medical problems is in a long letter on cholera of June 25, 1832. He had courageously returned to practice during the emergency created by the first great pandemic, and was now charged with a report. He felt that dietary indiscretions favored cholera. So did sudden changes in weather. The disease, unlike smallpox, killed primarily adults and left, therefore, a trail of orphans. He recommended the setting up of " Bureaux de secours " (Emergency Committees) which Paris had so successfully copied from St. Petersburg. The disease, if contagious at all, was so little contagious that contact with patients had not to be feared.

This anticontagionist attitude of Villermé does not surprise us. We know that he was the actual author of the famous 1827 Coutanceau report for the Academy of Medicine on the non-contagiousness of yellow fever.[8] Our letters reveal to us that he not merely agreed with the prophet of anticontagionism, N. Chervin,[9] but manifested to him a personal devotion we do not find anywhere else in the letters.

Je vous adresse un ouvrage d'un haut intéret. J'éstime singulièrement l'auteur et je l'aime. Comme notre censure n'en permettra qu'à peine l'annonce, si même elle la permet, Mr. Chervin y joint un second exemplaire. Je vous prie de donner celui ci au journal politique de Bruxelles le plus estimé. La personne que vous le portera est un de mes confrères. (August 9, 1827).

Besides shop talk the letters contain, of course, numerous comments on contemporary events. A letter of December 27, 1830, shows Villermé as a liberal in thorough sympathy with the French, Belgian, Swiss, and Polish uprisings of this year. The same holds good for the student riots of February 14, 1831, in favor of Poland and Belgium (letter of February 16, 1831). He served in the national guard in 1830. He was not an uncritical observer. Two passages give somewhat of the flavor of

[8] Ackerknecht, E. H., Anticontagionism between 1821 and 1887, *Bull. Hist. Med.* 22: 572, 1948.

[9] For Chervin, see Ackerknecht, 1948; interesting material on Chervin also in Rosen, G., An American Doctor in Paris in 1828, *J. Hist. Med.*, 1951, p. 88 ff.

a kingdom whose prime minister had coined the slogan "Enrichissez vous."

Ici tout s'exploite, tout est marchandise, jusqu'à la science, la philanthropie, la vertu et les vices; on en vend comme l'épicier du coin vend du sucre ou du poivre. (August 12, 1834)

Notre gouvernement ne serait pas moins bien disposé [d'advancer les statistisques]; toutefois la politique, les attaques, les tracasseries des deux oppositions, les intérêts des localités, les intérêts individuels, les entraves de toute sorte, l'arrêtent, ne le laissent pas reposer un instant et absorbent toute son attention, toutes ses ressources. Si, sous certains rapports, c'est une admirable chose que notre état constitutionel et les luttes qu'il produit, sous d'autres rapports c'est aussi un spectacle hideux; il fait ressortir toutes les mauvaises passions, toutes les cupidités. C'est au point qu'on se prend parfois a regretter le despotisme absolu. (May 24, 1842)

The same letter contains also comments on what apparently was the first railroad accident in France:

Non, personne des notres n'a été victime de l'épouvantable évenement arrivé sur le chemin de fer de Versailles. L'anxiété que nous vous avons donnée nous l'avons éprouvée aussi. Quoique je sois resté a Paris le 8, j'ai ressenti pendant quatre jours de suite un resserrement de poitrine, une oppression telle que la produirait un poids énorme. J'ai assisté à 20 batailles ou combats, et les milliers de morts ou de blessés que j'ai vus ne m'ont jamais, je crois, aussi péniblement affecté. Mais on se familiarise avec tout et la douleur générale qui a été occasionnée ici par l'horrible catastrophie est déjà singulièrement affaiblie. C'est que nous sommes dans un temps où les impressions se succèdent vite et se font non moins rapidement oublier, Cela n'empèchera pas d'ailleurs une seule fois les gens raisonables de prendre les chemins de fer.

The description of his new apartment, 32 Rue Vieille du Temple (August 17, 1843), is on the other hand full of those traits for which the French call this period "juste milieu," the Germans "Biedermeier."

[Nous sommes] dans le tranquille et classique quartier du Marais, où se retirent les petits rentiers, et tous les vieux bonhommes qui, fuyant le soleil, le bruit et les voitures, aiment les rues étroites, l'ombre et la boue.

Néanmoins les habitants du quartier qui s'ennuyeraient par trop de l'absence du soleil pourraient venir le voir chez moi; j'y ai même un jardin beaucoup plus grand que les plus grands jardins des fenêtres de cette capitale. C'est une vaste propriété ou paturent tout le jour des bêtes à cornes, elle a jusqu'à 3 allées dont 2 au besoin seraient assez larges pour un chat. J'oubliais de vous nommer les bêtes à cornes: ce sont des colimacons.

And yet less than five years later, in February, 1848, cannons were

fired in that calm, idyllic corner (letter of March 4, 1848) and in June the Villermés were surrounded by barricades and cut off for three days and nights (letter of June 30, 1848), and shot at when they appeared at their windows. The nearby house of a brother-in-law was used as an ambulance by the rebels. The son fought against the St. Paul barricade as a national guardsman and was on the proscription list of the rebels. The letter ends in a way characteristic for Villermé: "Je voulais vous écrire hier, mais j'ai dû courir toute la journée et cela m'a été impossible. Je suis même obligé, à l'instant, de courir pour un malheureux arrêté qu'il faut faire relacher; j'arrête cette lettre pour faire la course."

Villermé felt that the February revolution had been less spontaneous than it looked from the outside:

> Vous paraissez croire que la volonté et les calculs n'y ont été pour rien. Oui de la part du gouvernement, non de celle des masses et du parti qui triomphe. Les faits de corruption, l'achat de plusieurs écrivains, les prétendues missions données à un Alexandre Dumas, le procès Teste, l'advancement dans toutes les carrières accordé au sollicitations des députés dont on voulait acheter ou conserver les voix, et cela au detriment de ceux, qui y avaient le plus de droits, avaient jetté un profond mécontentment dans toutes les classes, y compris surtout l'armée. Voilà, mon cher ami, avec le comité directeur de l'opposition, ce qui a préparé et fait éclater la tempête. Ajoutez les journaux de l'opposition. (March 4, 1848).

Villermé had obviously set high hopes on the 1848 movement which ended in insurrections and the coup d'état of Napoleon III in December, 1852 (foreseen by Villermé in a letter of January 6, 1851). In one of his sad last letters (September 13, 1862) he explains that he doesn't mind dying: "Je suis triste aussi, surtout depuis 1848-1852."

Villermé's private life was, due to a modest fortune, relatively uneventful, dedicated mostly to his studies and his family. The letters show that, nevertheless, it was not without its tribulations like diseases, housing troubles, and difficulties in the administration of his estate, neglected too often for his scientific activities. Once he had to interrupt them for four years in order to make up in true César Birotteau fashion for the bankruptcy of his (later successful) son.

In 1851 he lost his wife through a cancer of the breast; in 1856 his son-in-law, de Frevilles, a promising medievalist, died suddenly. Villermé who was always deeply impressed by such losses and unable for months to see anybody, continued to live with his daughter and two small grandchildren. December 29, 1861, he complained: "Oui, le vide se fait autour de nous, à mesure que nous avancons dans la vie." He mentions the deaths

of Benoiston de Chateauneuf, Quetelet's pupil Verhuilst, Villot, Fourier, Dieterici, De Candolle, and Quetelet's daughter. Be it political disappointments, be it the personal losses, during his last fifteen years Villermé's letters show a certain decline in intellectual vigor and are more and more penetrated by sadness. In 1861 a rapid physical deterioration took place. He complained of great difficulties in climbing stairs, even in walking. He described graphically frequent and very painful attacks of "angine de poitrine." September 13, 1862, he sadly stated: "J'entends difficilement, je vois mal, je ne marche plus et je m'affaibli." Soon he became too weak to write. On November 16, 1863, his sufferings came to an end. Quetelet survived his friend by eleven years.

EARLY NEUROSURGERY IN NEW YORK: ITS ORIGIN IN NEUROLOGY AND GENERAL SURGERY

BYRON STOOKEY*

It is a significant observation that neurosurgery flourished only in those centers of general surgical renown where neurology was already preeminent. In New York the foundations of the specialty were laid by such distinguished neurologists as William Hammond, Edward Seguin, Charles Dana, Allen Starr, Bernard Sachs, and Graeme Hammond, working with a notable group of general surgeons, including Robert Abbe, Charles McBurney, Andrew McCosh, and Frank Hartley. That this city had early attracted an able and brilliant group of neurologists is attested by the fact that among their number were seven of the founders of the American Neurological Society and sixteen of the original thirty-five members of that organization.

Prior to discussing the observations of these men, however, it is pertinent to go back a little further and to consider some fundamental advances which had then only recently been made in the knowledge of the anatomy and physiology of the central nervous system. The new subjects of cerebral localization and spinal physiology had only begun to unfold.

Before it was recognized that definite functions are served by certain cerebral areas, the brain had been considered as a unit, functioning like any of the visceral organs. The principle of cerebral localization, first suggested by Gall (37) in 1809, and given more definite form in Bouillaud's (16, 17) claim (1825-1848) that derangement of speech follows a lesion in the anterior lobe, had been frequently challenged. This view had been advocated also by Marc Dax (28) in 1836, but his observations seem to have attracted little attention until their presentation by his son, G. Dax (28), in a Memoir published in 1864. Meantime the debate had been vigorously waged at a meeting of the Société d'Anthropologie in

* Charles A. Elsberg Lecture delivered at the New York Academy of Medicine, March 20, 1951, under the auspices of the New York Neurosurgical Society. It is a special pleasure to acknowledge my own great debt to Dr. Elsberg and to Dr. Alfred Taylor for the generosity of their teaching, and, through them, to pay homage to their predecessors, who, stimulated by the great contribuions of Macewen, Hughlings Jackson, Ferrier, Gowers, and Horsley, labored so faithfully and so well that New York City in the late 1880's and early 1890's was the richest center of neurology and neurosurgery in the United States.

1861. Gratiolet (41), representing the opponents of the theory, declared: "I do not hesitate to conclude that all schemes of localization hitherto proposed are without foundation. These are doubtless great efforts, titanic efforts, but when from the top of such a Babel we attempt to seize on divine truth, the edifice crumbles." On the other side of the argument, Auburtin (11), faithful disciple of Bouillaud, cited cases to prove his point, maintaining that, if instances of injury to the anterior lobes were discovered in which speech was not affected, these served only to prove that the part of the lobe concerned with speech was not involved. Going still further, he challenged Gratiolet to a definite test.

There was at that time on the service of Professor Broca (18) at the Hospital for Incurables (Bicêtre) a patient who had lost his articulate speech some years before. Auburtin agreed to renounce the doctrine of Bouillaud if autopsy failed to demonstrate a lesion in the anterior lobes of this patient. Broca, who previously had been hesitant to accept the doctrine of Bouillaud, agreed to the challenge, and when at necropsy a lesion was demonstrated in the third left frontal convolution, he became a most ardent advocate of the doctrine. Confirmatory evidence was obtained at a second autopsy, performed shortly thereafter, permitting a still more definite localization of the speech center in the posterior part of the third left frontal convolution.

In England, Hughlings Jackson (50) had become interested in the problem of cerebral localization, and his astute clinical and pathological deductions on the anatomical significance of localized seizures paved the way for galvanic stimulation of the cortex in lower animals by Fritsch and Hitzig (35) in 1870, and for Ferrier's (32) faradic stimulation of the motor area in monkeys in 1874 to prove that the motor centers were to be found in the ascending convolution. These views were subsequently brilliantly advanced by Horsley (47), Schaefer (47), and Beevor (13). Turck (78), in 1851, had demonstrated the relation of descending degeneration in the cord to cerebral involvement, and some six years later he described the essential lesion of tabes dorsalis as a sclerosis of the dorsal columns. In 1861 Brown-Séquard (19, 20, 21) pointed out the anatomical significance of a lesion affecting one-half of the spinal cord, and between 1873 and 1876 Flechsig (33) published a series of papers on the diseases of the spinal cord based on his own embryological and anatomical studies.

Hardly fifty years before this, the third edition of Wistar's (81) *Anatomy* (the first American anatomy), dated 1825, had contained only

the following description of the convolutions of the human brain: "The surface of the brain resembles that of the small intestine, or of a convoluted cylindrical tube; it is therefore said to be convoluted. The fissures between the convolutions do not extend very deep into the substance of the brain."

While knowledge of the cerebral and spinal functions evolved, Lister, at the Royal Infirmary in Glasgow, had made his great contribution to antiseptic surgery, so that the fatal infections which had previously followed almost any cerebral or spinal operation, could be avoided and the surgical procedures pursued with a reasonable degree of safety. Thus, thanks to the neurologists' advancement of cerebral and spinal localization, and the surgeons' development of antisepsis, the achievements of neurosurgery were made possible.

I wish that time permitted me to present more fully the biographical data on the distinguished neurologists and general surgeons who contributed so brilliantly to the progress of neurology and neurosurgery in New York. I shall offer only a few remarks on the two earliest and most eminent neurologists, who laid the foundations for their specialty in this city so firmly and broadly that those who succeeded them were able to carry their work to a high degree of perfection. I refer to William Hammond (43) and Edward Seguin (68, 69, 70, 71, 72) who in succession occupied the chair of neurology at the College of Physicians and Surgeons of Columbia University.

William Hammond of New York, one of the founders of the American Neurological Society, had been Surgeon General of the Army in which he instituted many reforms. He wrote a *Medical and Surgical History of the War of the Rebellion* and established the Army Medical Museum. He insisted on adequate housing for the sick and wounded, set up a permanent army hospital, now the Walter Reed Hospital in Washington, and founded the Army Medical School and Library. On his return to New York from his military duties, he was invited to join the faculty of the College of Physicians and Surgeons as Professor of Neurology. His textbook on *Diseases of the Nervous System*, in the writing of which his son, Graeme Hammond, collaborated, played an important role in furthering the study of neurology. This book, first published in 1876, had gone through nine editions by 1891, and had been translated into French, Italian, and Spanish.

Hammond was followed by Edward Seguin, one of the most brilliant and distinguished New York neurologists of the period. Seguin was

graduated from the College of Physicians and Surgeons at the age of nineteen and subsequently became Professor of Diseases of the Mind and Nervous System in that institution. After the Civil War he studied in Paris under Ronvier, Charcot, and Brown-Séquard. So distinguished a neurologist as Charles Mills of Philadelphia said that he had thought of Seguin as "the best neurological clinician he had ever known," high praise, as those who had the good fortune to know Charles Mills (60) will realize.

Seguin saw only a limited number of patients each day, so that they might be thoroughly studied; in the evening he elaborated his notes on his day's work. In 1883 he assembled his various original publications in a single volume, entitled *Opera Minora,* including one-hundred and eighteen articles on the nervous system, many of them of interest both to the neurologist and the surgeon. His studies on aphasia, published in 1888, were based upon some fifty cases critically evaluated, and this at a time when the problem of cerebral localization was only beginning to be worked out. In a series of lectures to his students at the College of Physicians and Surgeons during the winter of 1877-1878 he covered in a terse and systematic way the anatomical and physiological problems then known. He began his lectures with the spinal cord, then took up the brain stem and cerebellum, and concluded with cerebral localization. No more critical appreciation of the clinical significance of the physiology of the central nervous system and its direct application to disease had as yet appeared. Seguin's clinical approach was threefold, based on the following questions: (a) Is there disease? (b) What is the disease? (c) Where is the disease? These questions being answered the general problem then arose as to the procedure for the cure of the disease or for the relief of the patient.

Following Hammond and Seguin the chair of neurology at Columbia University was occupied successively by Allen Starr and Frederick Tilney. When the Medical School of Cornell University opened its doors in New York, Dr. Charles Dana was immediately selected to fill a similar position there. He had previously held the same appointment at Dartmouth, whose medical school is one of the oldest in the United States.

In addition to these men, Edward Fisher, Professor of Nervous and Mental Diseases in New York University, Edward Spitzka, at the Post Graduate, and Bernard Sachs, Professor in the New York Polyclinic and Chief of Neurology at Mount Sinai, brought to New York their penetrating insight into the diseases of the nervous system. To these names

should be added those of Pearce Bailey, Joseph Collins, Joseph Fraenkel, Smith Ely Jelliffe, and Abrahamson, all outstanding in a brilliant group.

Preeminent among the surgical group to whom we are indebted are Robert Weir, Arpad Gerster, Robert Abbe, Charles McBurney, Andrew McCosh, and Frank Hartley. Thanks to these men, at the turn of the century, the field was ripe for neurosurgical specialization and for the contributions of Cushing, Frazier, Elsberg, and Taylor. What neurological surgery needed most at this time was the exquisite refinement of surgical technic so carefully worked out and brilliantly practised by Halsted in Baltimore and applied with such success by Cushing to neurosurgery.

In conversing with my younger colleagues, I am disappointed to find them ignorant of most of these great names, and unaware of their indebtedness to the distinguished group of neurologists and general surgeons who, by their outstanding achievements, opened the field of neurosurgery to those in whose hands its development now lies.[1]

With the increasing advancement of neurology in New York there became apparent the need, first of a special service, and then of a special hospital, devoted to diseases of the nervous system. Dr. Bernard Sachs (67), following his graduation from Harvard and from the medical school at Strassburg, had the good fortune to be associated with Dr. Gerster, one of the great surgeons of New York, chief of the old German Dispensary and the first chief of surgery at Mount Sinai. Through his enthusiastic support, Sachs, in 1900, secured the assignment of twelve beds, six male and six female, as a neurological service—the first such service in this city. Dr. Sachs, who had been consulting neurologist, willingly stepped down to the position of attending neurologist for the sake of one active ward service devoted to neurologic cases. His wisdom and devotion to this service, and to his field, bore fruit in the development

[1] It is interesting to recall that when Cushing finished his interneship at the Massachusetts General Hospital and went to Johns Hopkins Hospital, he brought with him the concept of general surgery then in vogue at the Massachusetts General. As ward surgeon, Cushing waited on anxiously for his first patient to be brought down from the operating room after a four and a half hour operation. He had prepared the bed in shock position and was ready to administer stimulants, when Halsted asked whether anything had gone wrong to justify this unusual procedure. Having checked the pulse, temperature, and general condition of the patient and finding all satisfactory, he remarked on the hypodermic syringe of strychnine which Cushing, "having been educated in a school where memory and order were the rule," was prepared to administer—though for what reason he did not know. Halsted admonished him. Never do anything to a patient without understanding the why and wherefore was the lesson Cushing learned. (36)

of a number of eminent neurologists who received their training at Mt. Sinai.

In 1909, another forward stride in neurology was made, when Drs. Joseph Collins (24), Pearce Bailey (24), and Joseph Fraenkel (24) met to lay plans for the development of a separate hospital devoted to the diseases of the nervous system. Under the guidance of these men, the Neurological Institute was established, affording facilities for ward, semi-private, and private patients. At a meeting on May 20, 1910, the Medical Board of the Institute invited Dr. Elsberg to discuss with them the possibility of founding a surgical service, and within the next month the appointments of Dr. Elsberg and Dr. Taylor had been made. In July, Dr. Elsberg was again invited to meet with the Medical Board to present a table of organization of the surgical service. His plan was not finally approved, however, until December 30, 1910, approximately a year after the founding of the Institute, since no provision had been included for the two assistant surgeons, Dr. Edwin Beer and Harold Neuhof. This point was finally corrected, and by the beginning of 1911 the surgical service in the Neurological Institute had been established, with the addition of James Kenyon and Harold Burdick as assistants. With a special service for neurological patients at Mt. Sinai and the opening of a special hospital, the Neurological Institute, for the medical and surgical care of neurological patients, both the fields of neurology and neurological surgery in New York were well prepared for further growth.

Some of the earliest work in neurosurgery in New York was directed toward relief of the excruciating pain of trigeminal neuralgia. Indeed, the neurosurgical approach to this disesase may be said to have received its great impetus in the United States. The initial attempts for its relief consisted in peripheral section of the trigeminal divisions. This procedure was followed by intracranial section, removal of the gasserian ganglion, subtotal removal of the ganglion, dorsal root section with preservation of the motor root, and finally, differential section.

Carnochan (22), Professor of Surgery in New York Medical College, in 1856, developed an operation through the maxillary sinus to reach Meckel's ganglion and the second division of the trigeminal nerve, feeling that "this ganglion, composed of grey matter, must play an important part as the generator of nervous power, and that, unless it were removed, along with an inch of the second division, it were possible that the operation would not be successful." J. R. Wood (82) in 1866, followed by George Fowler (34) in 1887, Gerster (39) in 1896, and Abbe (6)—

all of New York—carried out numerous peripheral sections. Dennis (29) in 1879, collected and tabulated neurectomies involving the second division, together with removal of the sphenopalatine ganglion, according to the method of Carnochan. His report was followed by an exhaustive review and clinical study of eighty-six cases by George Fowler (34) of Brooklyn (1886), giving the name of the operator, sex and age of the patient, duration of the disease, date of operation, length of the segment removed, and duration of relief, together with comments. Fowler felt that "the results of neurectomy, although they do not bear out the brilliant anticipations once held, yet are such that, considering the violent suffering, neurectomy is not only justified but most imperatively demanded." His critical study stimulated further efforts to solve the problem.

The frequent recurrence of pain following peripheral section was thought to be due to insufficient removal, and intracranial section was advocated as offering more permanent relief. Abbe (5) of St. Lukes Hospital, after numerous attempts at peripheral section, exposed the branches intracranially and suggested that the interposition of gutta percha might prevent reunion of the divided ends and thus prevent a return of symptoms.

The inadequacy of this procedure and a better understanding of neuroanatomy led to a more fundamental approach to the problem. In 1891, Victor Horsley (48), James Taylor (48), and Walter S. Coleman (48) published an account in the *British Medical Journal* of an exposure of the gasserian ganglion through the temporal bone. By opening the dura and retracting the temporal sphenoidal lobe they were able to bring the ganglion and dorsal root into view. However, the attempt to strip off the ganglion along its upper half and near the cavernous sinus almost invariably resulted in tearing of the sinus. For this reason, Horsley concluded that complete removal of the gasserian ganglion was not practicable. "Finding this to be the case, I then considered the possibility of dividing the fifth nerve behind the ganglion." Having tried this operation on the monkey and also on the cadaver, he undertook it on December 11, 1890, on a patient with recurrent pain referable to the second and third trigeminal divisions following an earlier peripheral section. The temporal lobe was raised, the edge of the tentorium defined and the point at which the fifth nerve passed beneath it was seen. A small incision was made into the canal, the nerve was exposed and found to be lying free in the cavity. Horsley passed a small blunt hook around the dorsal root. It then occurred

to him that the small branches of the basilar artery, which accompany the nerve, might give some trouble, and he thought that in place of cutting the root it might be safer to attempt to avulse the nerve from its attachment to the pons. This was easily accomplished with no significant oozing, by gently drawing on the nerve with the hook. The wound was closed in the usual way. Unfortunately the patient failed to rally from the operation and died seven hours afterwards, from shock. Autopsy revealed only a slight amount of oozing in the subarachnoid space, but nothing to produce compression of any degree. Horsley was satisfied that the unfortunate outcome of this procedure was due to the special circumstances of the case. The patient had not eaten solid food for several months and was not in good condition to undergo the operation. Horsley had agreed to its performance because her condition was desperate and had warned her friends that there might be a fatal collapse even on the table. In spite of the unfortunate outcome in this instance, Horsley is to be credited with recognizing division of the nerve behind the ganglion as a more satisfactory procedure than removal of the ganglion itself. In a later communication Horsley refers to the fact that some fourteen years previously he and Macewen independently had performed the same operation "of dividing the dorsal root, thus making sure we cut off all possible paths between the ganglion cells and the central apparatus of the nerve."

Because of the unfavorable results of section of the divisions of the trigeminal, either intracranially or extracranially, another procedure was evolved. Working independently, Andrews (10) of Chicago (1891) and Rose (65, 66) of London (1892) developed an operation, which Rose was the first to perform. This consisted in section of the zygoma and coracoid processes and detachment of the masseter and temporal muscles, to expose the base of the skull and the foramen ovale. Trephining was then done in the vicinity of the foramen ovale to expose the gasserian ganglion; the third division and the second were sectioned. The ganglionic tissue, which is exceedingly soft, could then be pulled away piecemeal with curved hooks, or, as Andrews suggested, curetted. The dangers inherent in this procedure, in which the ganglion could not be adequately visualized, did not commend it as a satisfactory method.

It remained for Frank Hartley (44) of New York, to propose the procedure which in its essentials is still used for exposure of the gasserian ganglion. Hartley made a small omega-shaped incision just above the zygoma and turned down a flap including the skin, muscle, and bone,

FIG. 5.—Showing manner of holding the chisel in cutting the groove through the bone.

FIG. 6.—Flap elevated and turned down, exposing dura mater and middle meningeal artery.

FIG. 1

The first extradural approach to the Gasserian Ganglion. Frank Hartley, *Annals of Surgery*, 1893, 17 : 511.

exposing the ganglion by an *extradural* approach. (See Fig. 1.) The nerves were divided at the foramen ovale and rotundum, and the part between them and a point beyond the gasserian ganglion was excised. Hartley's first operation was done on August 15, 1891, and described before the New York Surgical Society on January 13, 1892 [2] and again in 1893.

Cushing (25, 26), some years later, referred to his approach as differing only in details from that devised by Hartley.

The further evolution of the operation for trigeminal neuralgia is historically of great interest. Horsley (46) and Macewen, independently, were the first to conceive of and to attempt dorsal root section. Ferrier (31), in discussing Rose's first operation for removal of the gasserian ganglion, stated that " section of the nerve above the ganglion might cause an ascending degeneration and thus lead to relief by producing atrophy of the center."

Subsequently, Spiller (51), in 1898, suggested sectioning the dorsal root rather than removing the ganglion, and this proposal was repeated by Lewellys Barker (12) in 1900, who said, " In tic douloureux relief should be as complete and permanent from cutting the nervus trigeminus between the ganglion and the pons and avulsing the central end, without removal of the ganglion, as when the ganglion itself is excised." It remained, however, for Frazier (74), at Spiller's (74) suggestion, October 12, 1901, to section the dorsal root successfully without attempting to remove the ganglion. The two earlier cases of dorsal root section by Macewen and Horsley, mentioned above, were done through the more hazardous *intradural* approach and, unfortunately, resulted fatally. Had the extradural route, suggested by Hartley, been used, a more favorable result might have been obtained.

An additional refinement of the operation was proposed by Tiffany (77) in 1896. He advocated subtotal resection, explaining his position as follows:

> It may be accepted without hestitation that the ganglion and branches can be removed after being clearly exposed. I doubt the expediency of so doing. (1) The first branch is never affected alone; it may be involved reflexly. (2) Trouble of one kind or another has been often met with when the first division is removed.

[2] Fedor Krause (53, 54) independently employed a similar approach, February 23, 1892, and described it in the *Archiv für klinische Chirurgie*, October 11, 1892. The procedure came to be known as the Hartley-Krause method but the priority in its use clearly belonged to Hartley.

. . . Involvement of the eye shows itself, as anesthesia of the cornea, requiring protection for a long time, panophthalmitis, etc. These are the two main reasons why it is well not to take away the upper portion of the ganglion and the first branch, but rather to remove the second and third branches with the corresponding portion of the ganglion. . . . Entire removal may not be necessary, and so involvement of the eye in a destructive inflammation may be avoided.

Tiffany also was the first to suggest saving the motor root. "The expediency of attempting to save the motor fibers accompanying the third branch may be considered. Usually I have not taken the pains to do so. . . . While the necessity for a bilateral operation is remote, yet the Hartley-Krause operation will be more complete if the motor root is spared, and I think it can be."

From the foregoing review, it is apparent that many New York surgeons, from Carnochan (1859) and Wood (1868) to Fowler (1885), Abbe, and Hartley, were actively concerned with the important neurosurgical problem of affording relief from the dread pain of trigeminal neuralgia. Hartley's extradural approach made possible the low mortality rate which the subtemporal trigeminal operation now enjoys. Only recently, however, a visiting colleague urged adoption of the intradural approach, ignorant of the fact that it had been tried and long since discarded.

The earliest operations on the spine were done for fractures and for Pott's disease. The first laminectomy of which I have been able to find any record was performed in 1814 by the younger Henry Cline (23) at St. Thomas' Hospital, London. The eleventh and twelfth spines and one of the arches were removed for fracture of the vertebrae. Cline's surgical judgment in proposing operation was clearly presented:

In fracture, with displacement of the vertebrae which compresses the spinal cord, this great nervous cord is under precisely the same circumstances as the brain when pressed by the fractured skull, and therefore, as the elevation of the pressing bone is indicated in the latter case, and often effected with success, so is it equally called for in the former, and no positive reason could exist why the operation should not be successful, provided the injury to the spinal cord itself were not a great condition, which, as regards the brain, would equally forbid operation or render its success improbable or impossible.

Many New York surgeons exposed the spine for fracture. Among the earliest who reported this procedure were Hutchinson (49) 1861; Potter (63) 1863; Halsted (42) 1885, Keyes (52) 1885; Morris (61) 1885; Dawbarn (27) 1889; Abbe (1) 1889; Richardson (64) 1889; and Pilcher (62) 1890.

It was long known that paralysis due to Pott's disease was usually not the result of direct involvement of the spinal cord but was caused by and was proportionate to the degree of pressure exerted on the cord by the exudate and, in rare instances, by the diseased bone itself. Percival Pott had noted that "the useless state of the limbs is by no means a consequence of the altered figure of the spine, or of the disposition of the bones with regard to each other, but merely of the caries" and centuries earlier Hippocrates and observed: "The palsy of the legs and arms, the wasting of the body, and the retention of the urine happen chiefly when there is no spinal deformity . . . and less frequently in cases of gibbous or angular deformity." Abbe (4) 1890; Wyeth (83) 1890; and Gerster (38) all reported relief of symptoms of spinal cord pressure in the presence of Potts disease by laminectomy. Fortunately, this is now a rare affliction and pressure on the cord a still rarer complication.

Before Lister's antiseptic regime, spinal surgery had most often been followed by meningitis and death; hence any operation upon the spine, as upon the brain was regarded as a most hazardous procedure. This view unfortunately tended to persist into the period when the operation could be safely done. It happened, however, that Lister's earliest contributions were made in Glasgow, where Macewen, a surgeon of great ability, was at hand to seize upon them. This fortunate coincidence accounts in part for Macewen's early successes. It was also to his advantage to be associated at the infirmary with a distinguished neurologist, Dr. Alexander Robertson, who, as early as 1866, was interested in neurophysiology and localization of cerebral function.

In 1888 Macewen reported on six spinal cord operations, two of which had previously (1885) been presented before the Glasgow Pathological and Chirurgical Society. Macewen (58) held that "the spinal membranes and the cord itself can be exposed, and neoplasms and encroachments upon the lumen of the canal may be removed therefrom without unduly hazarding the life." He quoted those opposed to laminectomy as contending that laminectomies are, first, full of danger, being difficult, prolonged, and attended by profuse hemorrhage; that the operation could hardly benefit the patient; and that no one had yet been able to present a successful case. These opponents to spinal surgery, Macewen added, were "so hampered by the inculcated physiological dogma of the time, that the true significance of the neurological signs never dawned on them. The facts were reflected from their brain as objects from a mirror, and no more."

Horsley (40) was the first to remove an *intradural* spinal cord tumor, on June 9, 1887. His report was read before the Royal Medical and Chirurgical Society of London, a year later, June 12, 1888. The first removal of a tumor of the spinal cord in New York was done by Abbe (1), May 26, 1888, without knowledge of Horsley's achievement.

Horsley assembled from the literature fifty-eight examples of spinal cord tumor showing that in 74 of the extradural and 83 of the intradural cases the patient died from the indirect effects of the tumor, such as septicemia from bed sores, acute nephritis from urinary retention, and pneumonia. Thus, in some 80 of the cases relief might have been given by surgical intervention.

Some of Horsley's remarks in connection with his own case have an all too familiar sound to the neurosurgeon in search of a spinal cord tumor. He first removed the spines and laminae of the fourth, fifth, and sixth thoracic vertebrae but, on opening the dura, he found no tumor. A lamina above and a lamina below were then removed

> but still nothing pathological was found. . . . At this juncture it appeared as if sufficient had been done, but I was unwilling to leave the matter undecided, and my friend, Mr. Ballance, being sharply of the opinion that further exposure of the cord was indicated, I determined to go further. I removed another lamina at the upper part of the incision. On opening the dura mater, I saw on the left side a round, dark, bluish mass about three millimeters in diameter. . . . I recognized it at once to be the lower end of a new growth, and, therefore, quickly cut away the major part of the lamina next above. This enabled me to see almost the whole extent of the tumor when the dura mater was divided. The growth was found by measurement to be four inches above the level of complete anesthesia.

What neurosurgeon has not had this experience in spite of every clinical effort to establish an exact level preoperatively!

In Abbe's case the spines and laminae of the eighth, ninth, and tenth thoracic vertebrae were removed, exposing a dense mass of tissue compressing the cord. At the time of his report before the New York Medical Society in February 1889, his patient was able to walk without support, though he had previously been paralyzed, anesthetic, and incontinent of urine. The wound was well healed. The patient remained well for two years; then, some discharge appeared in the wound, and the mass was consequently assumed to have been tubercular.

A second spinal cord tumor was removed by Abbe in April, 1890. This lay extradurally beneath the arches of the eighth, ninth, and tenth vertebrae, extending between the arches and pedicles and into the sub-

pleural space. Unfortunately, operation had been postponed so that at the time it was performed the patient was suffering absolute anesthesia below the waist, complete paralysis, incontinence of bowels and bladder, acute nephritis, with 20% albumin in the urine, chills, and fever (102.2°).

A

CONTRIBUTION

TO THE

SURGERY OF THE SPINAL CORD.

BY

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FIG. 2

Operation was undertaken with a full realization of the handicap presented by the unfavorable general condition. All went well, however, after removal of the tumor until the fifth day, when vomiting and hiccoughing began and continued with increasing severity. From a surgical standpoint the tumor had been removed, and the patient had withstood the immediate effects of the operation well.

Seguin had referred this patient to Abbe, giving an analysis of the

neurological signs leading to the diagnosis. As an example of the clear and concise clinical reasoning so long emphasized by Seguin, as Professor of Nervous Diseases in the College of Physicians and Surgeons, I cite part of his summary:

> The diagnosis of compression myelitis was arrived at by deductive and exclusive reasoning. The dominant symptom, the "signal symptom," was the fixed side pain which soon became lumbo-iliac (?) pain. Then followed a gradually increasing paraplegia, without atrophy (at first), but with great increase of the reflexes. The anesthesia was secondary during the first few months.
>
> Now, this symptom group (at the end of October, 1889) was suggestive of a myelitis in the involvement of the lateral columns; a spastic paraplegia plus anesthesia. The anesthesia served to exclude primary sclerosis of the lateral columns. But the initial symptom, the fixed side pain, has, we well know, a very restricted meaning. It is never present in myelitis, or in sclerosis of any system of the cord. It surely indicates an extracordal lesion, involving in some way the posterior root or the mixed root of the nerve which supplies the seat of the fixed pain. I call this the dominant symptom because it serves to locate the lesion (or the chief and primary lesion) outside the spinal cord at a certain level. This being granted, the other symptoms, which are apparently more striking, viz. slowly increasing paralysis and anesthesia, (the latter subordinate) and great increase of reflexes, would surely point to the existence of a lesion outside the cord, producing steadily increasing pressure on this organ at a limited point or level, leaving a healthy segment of gray matter caudad of the lesion. The diagnosis turns upon the pressure of fixed pain in one side, and also, though with much less logical force, upon the comparative slight anesthesia, at a time when the volitional motor impulses were wholly arrested at a given level. . . .
>
> What I have said of the significance of side pain will explain why I have always taught that great care should be taken to ascertain exactly whether the sensation in the patient's side (chest, abdomen, groin) is a true pain, or a sense of constriction. The former has the specific value I have assigned it above, while the latter occurs in myelitis, meningo-myelitis, posterior spinal sclerosis, . . . etc. (4)

Neurology and neurosurgery in New York may well be proud of another achievement, namely, intradural section of the dorsal spinal root for relief of intractable pain and also for athetosis, proposed by Dana and first performed by Abbe (2, 3, 7) in 1888. Abbe's (1) report appeared in 1889, for which both he and Dana deserve great credit—credit which has not been given though the record clearly indicates their priority in this procedure. In 1890, Abbe (3) reported a second dorsal root section, performed in February, 1889, in which the sixth, seventh, and eighth cervical and first thoracic dorsal roots were cut. In spite of the published records of these two operations, Spiller (73) and Frazier (73), in their report in 1910, credit Spiller with suggesting dorsal root section in

1905—more than fifteen years after Abbe's report—though the operation was not done by Frazier until five years later. The report of Abbe and

Intradural Section of the Spinal Nerves for Neuralgia

Compliments of
Dr. ROBERT ABBE,
13 WEST 50th ST.

BY

ROBERT ABBE, M. D.
NEW YORK

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FIG. 3

The first intradural section of spinal nerves for neuralgia. Robert Abbe, *Transactions of the New York State Medical Society*, February, 1889, and reported again in *Boston Medical-Surgical Journal*, 1895, 135: 329-335.

Dana, so long overlooked, is worthy of being again brought to the fore and placed on record.

This operation was also notable as representing the first attempt in a human being, without anesthesia, to stimulate the exposed spinal nerves with faradic current, and observe contractions of the muscles supplied by

the various spinal segments. At a second stage, additional nerve roots were sectioned as well as stimulated, while the patient was conscious, so that sensitivity of the dura and the spinal cord could be determined.

RESECTION OF THE POSTERIOR ROOTS OF SPINAL NERVES TO RELIEVE PAIN, PAIN REFLEX, ATHETOSIS, AND SPASTIC PARALYSIS— DANA'S OPERATION.*

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It is more than twenty-two years since Dr. C. L. Dana asked me if I were willing to perform for him an operation which had not heretofore been done and which he believed offered relief to a patient then suffering violent reflex neuralgic pains of the right arm, with muscle contraction associated with the spasms of pain, who already had had nerve stretching and amputation at the upper humerus by Dr. Bull with no abatement of pain. He conceived that the operation of "cutting the posterior roots within the dura would cause ascending degeneration and destroy the sensory tract, even into the spinal cord." The plans for this unique operation were made, as shown by Dr. Dana's letter to me in September, 1888. The operation was delayed by the patient's desire to have a neuroma of his stump resected first.

With every precaution I operated December 31, 1888, assisted by Dr. Bangs and in the presence of

*Read at a meeting of the Practitioners' Society of New York, January 6, 1911.

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FIG. 4

The first resection of the posterior roots of the spinal nerves for the relief of athetosis and spastic paralysis. Robert Abbe, operation done 1888, reported in 1889 and 1896, and attention called to it again in 1911. Reprinted from *Medical Record*, 1911, 79: 377-386.

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A statement previously made, that the dura was sensitive, was then disproved.

It would be impossible to conclude any remarks concerning spinal cord surgery in New York without referring to the brilliant contribution

of Alfred Taylor (76). Hemilaminectomy as devised by Dr. Taylor unfortunately met with such opposition when first presented that it had been practically abandoned when I had the good fortune to be associated with Dr. Taylor. I asked him if he would demonstrate the operation to me, and after seeing the beautiful exposure obtained in both the cervical and lumbar region when the proper technic was used, I was so enthusiastic that he was persuaded to undertake the procedure again, and performed it on a number of patients, while I worked with him at Bellevue and the Neurological Institute. It is a great tribute to him that this operation is, I believe, now made use of in most neurosurgical clinics.

In the cervical region, the area of dura exposed is practically equal to that obtained by a bilateral exposure. Through a hemilaminectomy I have been able to take out, without damage to the cord, the largest ventrally located meningioma I have ever removed. Taylor's hemilaminectomy is particularly desirable as an exploratory procedure and in the presence of ventrally placed tumors or herniations. Its advantages from a structural standpoint are obvious,

In surgery of the brain, as of the spinal cord, Macewen (59) was the pioneer. The first successful removal of a brain tumor was done by him in Glasgow in 1879. The bone over the left frontal region was thick and a soft flattened tumor of the dura, extending to the orbital plate, was removed. The patient was restored to perfect health and able to earn his living until he died some eight years later of chronic Bright's disease. Autopsy showed no recurrence of the tumor. Prior to 1883 Macewen had successfully performed six additional cerebral operations, one for brain abscess in 1881.

However, one of the earliest brain operations in New York was performed by William Detmold (30), 1850, for brain abscess. This case is all the more remarkable in that it was done years before Lister's antiseptic methods, and before cerebral localization had been established. Detmold's patient had received a blow on the left frontal region followed by necrosis of bone. The patient became stuporous, the necrotic bone was removed and an incision made into the substance of the brain about one inch in length and about one-half inch in depth. This was followed instantly by a thick stream of healthy pus, some 60 to 150 cc. of pus being evacuated, whereupon the patient opened his eyes, said he felt better and put out his tongue when requested. The wound healed with the exception of a small sinus and three weeks after the operation the patient was out of bed. This was followed by a period of comparative health, except for aphasia.

However, some thirty-seven days after the operation stupor again came on with headaches and tremors, re-accumulation of the pus was suspected, but incision of the brain 3 cm. deep evacuated none. Shortly thereafter the patient became moribund. Detmold made a last attempt to relieve him by making an incision into the lateral ventricle, evacuating pus freely. Seven hours later, however, the patient died and post-mortem showed both lateral ventricles containing pus, the abscess having evidently ruptured into the ventricles.

In London, in December 1884, Mr. Godlee (14), assisted by Dr. Bennett and Dr. Ferrier, operated for a tumor in the upper third of the motor strip, removing a glioma the size of a walnut. The bleeding was checked with galvanic cautery and the cavity drained. Unfortunately, four days later suppuration, hernia cerebri, and meningitis ensued, and death followed four weeks after operation.

However, even before Bennett's and Godlee's operation, R. W. Amidon (8) of New York read a paper before the New York Academy of Medicine, June 5, 1884, in which he entered a plea for more heroic surgical interference in affections of the brain. He urged that "accessible neoplasms of the brain, which have resisted medicinal treatment, and which continue to grow and threaten life, should be removed; for the reasons that they are generally single, seldom have secondary deposits, are surrounded by an inflammatory zone of demarcation, and *always* kill by pressure."

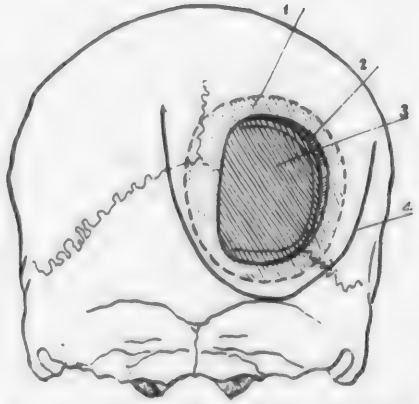
The following year, in another contribution, Amidon (9) reminded his readers that only a few months previous he was credited with holding advanced, radical views and advising more heroic procedures than the existing state of skill would warrant. Yet, within seven months of the time at which he ventured the assertion that some kinds of cerebral tumors could be excised, there came the news from England that a previously diagnosed tumor of the brain was removed by a skillful surgeon, Mr. Godlee, whose hand was guided by two eminent neurologists, Dr. Bennett and Dr. Ferrier. In their report no statement was made that provision for free drainage was established and Amidon felt that if this was neglected it was "a grievous error," inasmuch as after checking hemorrhage by cautery there is bound to be certain sloughing necrosis which, if confined, may induce septic conditions.

In New York the first brain operation, with *complete* removal of a tumor, was done by Robert Weir (15), March 9, 1887, in the presence of a distinguished group of neurologists and surgeons, among them Drs.

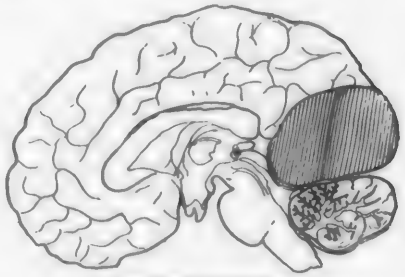
Birdsall and Seguin as neurological counsel, and Drs. Dana, Mills, Sachs, Starr, Abbe, Bull, Hamilton, Markoe, Olcott, and others. Dr. Charles Mills, referring to the occasion, wrote: "On the invitation of Dr. Seguin I went to New York to witness the first operation for a tumor of the brain by an American surgeon, Dr. Weir."[3] The patient was first seen by Seguin in November, 1885, when a diagnosis was made of a tumor of the mesial aspect of the right occipital lobe involving primarily the cuneus, and extending downward toward the tentorium, and upward toward the parasagittal lobule. Subsequently, the patient saw a number of other physicians. He finally came to operation some fifteen months later, at which time pressure symptoms were rapidly increasing.

A U-shaped flap, three inches long and three inches wide, was made, under a carbolic spray, over the right posterior cerebral lobe, the medial line crossing the midline. The trephine opening was made and the bone rongeured away until an oval opening measuring two and three-quarters by two and one-quarter inches was effected. The dura mater, non-pulsating, rose tensely in the space and was of deeper hue than normal. On opening the dura, a purplish-red tumor, with large veins ramifying in it, was encountered. The bone opening was enlarged, but not sufficiently to reach the edge of the tumor. It was therefore incised and some of the softened, fatty-looking, granular contents removed. This diminished the side of the tumor and enabled the forefinger to be passed between it and the cranium. By drawing the finger gently toward the cranial opening, the tumor was torn nearly in two and its outer half lifted out after which the inner half was separated from the falx, to the posterior border of which it was attached, and withdrawn by the end of the finger and the nail. Inspection showed that the removal was complete. Bleeding was controlled with sponges and temporary pressure. The cavity in the brain indicated the immense pressure exerted by the tumor which weighed 140 grams and measured 8½ inches in its greater circumference, 7 inches in its lesser. The cavity was packed with iodoform gauze, over which the dura was partly united by several loose sutures, instead of being closely brought together. The operation was well borne until

[3] Since this operation took place shortly after the beginning of antiseptic surgery, Dr. Weir had a house officer stationed in the operating room whose sole duty was to watch the surgeons and nurses to make sure that there was no break in technic, and if a break in technic was seen, the house surgeon was instructed to speak up loudly and say, "Error." During this operation in the presence of such a distinguished group, the house surgeon called out, "Error, Dr. Weir" when Dr. Weir started to push away a lamp which was too close to the field.



Field of operation. 1. Extension of tumor at its deepest part. 2. Opening in the skull (2¾ x 2½ inches). 3. Flap of dura turned toward the median line. 4. Flap of the scalp.



Vertical section showing tumor.

Tumor, seen from its base; actual size.

FIG. 5

The first complete removal of a brain tumor in America. Robert Weir, *Am. Journ. Med. Sc.*, 1888, New Series 95: 109-128.

the final enucleation, when the pulse fell from loss of blood. The limbs were bandaged and whiskey given subcutaneously, and at the close of the operation the patient was in fair condition with a pulse of 132 which was regular and of good volume. On coming out of ether he moved all his limbs but was restless; his voice was normal. Two hours after the operation the pulse became slower but weaker. At six hours, the patient was conscious but dull. He was given a salt transfusion with sugar, but failed to respond and died shortly thereafter.

Considering its location at the confluence of the venous sinuses, its attachment to the falx, and its enormous size, removal of this tumor, even with the great advances which have since been made in neurosurgery, would entail considerable hazard. It is unfortunate that the first such operation in New York should have been in so difficult a case. The problems presented were far greater than had been encountered by Macewen or Godlee in their cases. In the previous year (1886) Hirshfelder (45) and Morse of San Francisco had operated for infiltrating tumors, but as in the case of Bennett and Godlee, removal was incomplete.

In the report of his case, Weir (79) made a number of critical remarks concerning the problems encountered at operation. As early as 1882, and twice since then, he had come upon lacerated vessels in the brain substance which he had controlled by ligature or by torsion, but none of these were at a greater depth than an inch. Another time he would not rely upon pressure alone to stop bleeding but would use a clamp forceps which might be allowed to protrude through an opening in the wound for twenty-four hours. He also felt that elevation of the head would help to control bleeding, though, because of shock, he had directed that the head be lowered. The opening in the skull, he felt, should be large and might be covered by bone grafts as recommended by Macewen, or by sprinkling bone chips over the closed dura, as done by Horsley, or by replacing the trephine discs as he himself had previously done when operating for epilepsy—a procedure also followed by McBurney. In concluding, Weir stated that, with localization, so ably done by the neurologist and physiologist, surgery would be more and more available for the rescue of patients otherwise condemned to paralysis and death.

Birdsall, considering Weir's case from a neurological standpoint, commented upon the absence of severe headache, usually a prominent symptom in meningeal tumors, and stated that recurring growths, limited to the meninges, may attain large size before disturbing the function of the neighboring parts of the brain. These frequently give rise to less marked

symptoms than very small growths which infiltrate the cortex. The remarkable feature of these large tumors is not so much that they give rise to localizing signs as that they produce so few symptoms.

Seguin (79) also commented from the neurologist's point of view. He insisted that a diagnosis of brain tumor should be worked out in not less than five lines of inquiry as follows:

1. The diagnosis of a tumor of the cerebrum
2. The diagnosis of the exact location of the tumor
3. The diagnosis of the depth of the tumor
4. The diagnosis of the solitude or multiplicity of the tumor
5. The diagnosis of the nature of the tumor

To the discussion of these five ends he brought a keen mind, well versed in neuroanatomy and neurophysiology and skilled in deductive clinical reasoning.

The impetus given by Weir's procedure to the neurologists and surgeons of New York led to a number of brain operations. In November of the same year Weir himself successfully operated for a tumor deep under the surface of the brain in the face and arm center, the precise localization having been made by Seguin. Using retractors made from bent spoon handles, Weir exposed and removed the mass. The dura was closed and the bone discs replaced to cover the cranial opening. On the sixteenth day the patient was out of bed, and six months later sailed for Europe, apparently in perfect health. In a previous operation, October 11, 1886, Weir found no tumor, nor could one be felt with the finger. A needle which was then inserted in several directions, and at varying depths, failed to encounter resistance. Similar explorations were reported by Gerster and Markoe. In an operation by Hammond four trephine openings were made by an electro-trephine and connected by straight lines with a circular saw.

In the late 1880's and early 1890's numerous operations for epilepsy were performed in New York by Abbe, Weir, Gerster, McCosh, McBurney, Briddon, Woolsey, Bryant, Hartley, and others in cooperation with Seguin, Starr, Sachs, Dana, and Fisher. The problem was of special interest to both Sachs and Starr, Sachs' cases being done by Gerster and most of Starr's by McBurney. Starr (75), in 1893, collected the cases published up to that time in this country, and included a careful analysis of the clinical signs, surgical findings, and pathological material removed in thirteen of his own cases, the pathology of which had been thoroughly

studied by van Gieson. In the entire series of forty-two cases there were thirteen cures; eleven patients were improved, fifteen unimproved, and three died. Starr concluded that, taking the series as a whole, it was evident that in the majority of instances the operation had been a failure, that this failure was due to inability to eradicate completely the underlying brain disease, and that excision of diseased brain was inevitably followed by a new cicatrix, which in turn acted as an irritant. He felt, nevertheless, that operation should be considered and, in selected cases, performed.

In 1888 Weir (80) reported on his experiences in seven personal cases, three of tumor, three of cerebral abscess, and one of epilepsy, which, together with his study of other published cases, led him to appreciate the value of the following points in brain surgery:

1. Scalp bleeding may be lessened by transfixing the scalp parallel and just exterior to its edge with acupressure needles, or by tying a rubber band about the head as suggested by Allen Starr.

2. A minute perforation of the scalp should be made and then a mark put on the bone at the exact spot to begin the trephine.

3. The cranial opening should be a large one to permit adequate exposure.

4. The dura must be opened. Weir condemned the practice of making a diagnosis of tumor by inserting a needle through the unopened dura, nor under these circumstances, is the needle a safe instrument to use.

5. Bone fragments may be replaced, as done by Macewen and Horsley, but bone trephine discs are preferable as replacements.

6. The handle of a spoon, the edges of which are rounded, may be used as a retractor to investigate the depth of the brain.

7. It is better to keep the head elevated for a few hours after operation to lessen bleeding.

Within the next few years an intensive study of cerebral tumors and their surgical removal was made by Starr (55, 75), McBurney (55), and McCosh (56). In 1893, McBurney (55) and Starr (55) presented a paper based on the diagnosis, localization, and removal of three tumors of the brain. McBurney pointed out that an exploratory brain operation differs from many other exploratory procedures in that unexpected difficulties, even serious accidents, are likely to occur at almost any stage, so that the exploration itself is an operation of importance. Hemorrhage

is a vital consideration, and may be unexpectedly increased in the later stages, when the patient has already lost a considerable quantity of blood.

The successive steps of a cerebral operation were described in some detail. The incisions in the scalp should be free so as to lay bare a large area of bone—at least three inches in diameter. The skull should be opened in the line of the scalp and the bone flap, with the scalp flap attached, should be turned back. The bone flap should be lifted slowly to make sure that vessels or adhesions are not rudely torn. Bleeding from the dura may be controlled by a light touch of the Paquelin cautery. The dura is opened in the same shape as the bone flap. Vessels on the surface of the brain may be contracted by cautery. The dura may be closed or left open and the bone flap replaced.

McCosh (57), in 1894, reported four operations on the brain. In one of these, since no tumor was found after a thorough exploration and there was extreme pressure, the ventricle was tapped and an ounce of thin, straw-colored fluid was removed. A trocar and cannula were then thrust into the ventricle and the cannula left for permanent drainage. It was carried out through an opening made in the middle of the flap, fixed so that it could slip neither out nor in, and was removed after forty-eight hours. The patient recovered and returned to work. At the time of the report, seven months after the operation, he had not missed a single day on his job.

In the preceding discussion I have sought to present the background of neurosurgery as it was laid by the neurologists and surgeons of an earlier day. It is these men who, by their own experience and their observations of the work of others, laid down the basic principles of brain surgery. Briefly these tenets may be summarized as follows: Adequate exposure; the formation of an osteoplastic flap; use of an electric trephine and saw; the recognition that even an exploratory operation may be hazardous; the necessity of special attention to hemorrhage, which in the latter stages of an operation, when the patient has already lost a considerable amount of blood, may prove a serious matter; the use of the cautery to control bleeding; the reduction of increased intracranial pressure by tapping the ventricle; temporary ventricular drainage; replacement of bone strips, chips, or trephine button; and consideration of the position of the patient's head after operation.

Along with these basic neurosurgical principles went a wealth of unexcelled neurological clinical judgment furnished by a highly distinguished group of neurologists. In this rich New York surgical and

neurological environment, Charles A. Elsberg, whose memory this paper is to honor,[4] began his surgical career when he graduated from the College of Physicians and Surgeons, Columbia University, in 1893.

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[4] See footnote, p. 1.

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DR. NATHAN SMITH DAVIS AND THE GROWTH OF CHICAGO MEDICINE 1850-1900

THOMAS N. BONNER

Chicago had already begun its meteoric rise to national prominence when Dr. Nathan S. Davis arrived there in the late summer of 1849. Both the city and the doctor were young; a persistent vivacity and youthfulness were to characterize the activities of both in the succeeding half-century. Of the two, the physician was the older, having been born in 1817, while Chicago dated its incorporation as a village from the year 1833. Boosters of the new entrepôt of the Old Northwest (like the fabulous John Stephen Wright, great grandfather to all Chicago boosters and boomers) could point with pride to population and trade statistics which seemed to assure a mighty and unprecedented future for their city.[1] Partisans of Dr. Davis, on the other hand, could boast that their candidate for the vacant chair of physiology and general pathology at Rush Medical College had already accomplished more by 1849 than most of his professional brethren in a lifetime.

Dr. Davis had indeed come far since his farm boyhood in Chenango County, New York. While yet in his teens he commenced reading for a medical career with one of the local doctors, and at the age of twenty he graduated from the College of Physicians and Surgeons at Fairfield, New York. This institution, one of the better staffed and equipped of the "country medical schools" of the early nineteenth century, was also the *alma mater* of many other pioneering Chicago physicians.[2] Characteristically, young Dr. Davis not only survived a continuous fare of roast potatoes, pudding, and milk at Fairfield, but also "saved enough to buy me a small library of books and a pocket case of instruments to commence practice with."[3]

[1] The most lively account of the early history of Chicago is in Lloyd Lewis and Henry Justin Smith, *Chicago; the History of Its Reputation* (New York, 1929).

[2] William F. Norwood, *Medical Education in the United States before the Civil War* (Philadelphia, 1944), 150-53; George H. Weaver, "Beginnings of Medical Education in and near Chicago; the Institutions and the Men," *Bulletin of the Society of Medical History of Chicago*, III (Sept., 1925), 341-42. Factual data concerning Davis' life, unless otherwise indicated, are taken from I. N. Danforth, *The Life of Nathan Smith Davis* (Chicago, 1907).

[3] Nathan Smith Davis III, "Nathan Smith Davis, M. D., A. M., LL. D.," Morris Fishbein (ed.), *A History of the American Medical Association 1847 to 1947* (Philadelphia, 1947), 4.

The years between his graduation in 1837 and his coming to Chicago twelve years later were spent in rural practice in western New York. He became a member of the Broome County Medical Society, where he was active on committees concerned with scientific literature, quackery, and medical licensure. Several articles were published as a result of his investigations in these fields. In 1842 he served on a committee charged with examining the botany of Broome County with an eye to pharmaceutical possibilities.[4] The physician of Davis' day was conversant not only with medicine, but frequently with the whole range of natural sciences, including geology, zoology, botany, or even geography and agronomy. Davis himself published, in 1848, an informed textbook on agriculture.[5]

But it was his efforts to establish a national medical association that attracted most attention to the Broome County doctor. Davis, like many of his contemporaries, deplored the low standards observed by medical schools and medical societies, the two agencies empowered to grant licenses to practice. The schools were engaged in a vigorous competition for students and the resulting commercial emphasis did much to retard any genuine efforts at reform. Beginning in 1844, Davis began to urge that a national convention of physicians be called to consider the problem. Two years later, largely through his persistence and initiative, an organizational meeting was held in New York City. An attempt to adjourn the gathering *sine die* failed, and Davis was appointed chairman of a committee to draw up suitable proposals. These were approved and subsequently re-affirmed the following year at Philadelphia, where steps were taken to effect a permanent organization. The name of "American Medical Association" was decided upon at the latter meeting.[6] As a result of the publicity attendant upon his work in forming the new society, Davis was offered and accepted, in 1849, a chair at the Rush Medical College in Chicago.

Chicago in 1849, though in process of rapid development, presented many unwholesome aspects to an observant young medical man. There was, as yet, no permanent general hospital in the city and no provision had been made for the care of the destitute. There was no adequate supply of good water; no satisfactory system of sewerage had been developed.

[4] Charles S. Marshall to Nathan S. Davis III, Apr. 24, 1943, MS., Church Library, Northwestern University Medical School. Dr. Marshall is here reporting the results of a search of the records of the Broome County Medical Society concerning Dr. Davis' activities.

[5] N. S. Davis, *A Text Book on Agriculture* (New York, 1848).

[6] M. Fishbein, *History of the American Medical Association*, 21-31.

Moreover, what could be said for a town that allowed shiploads of immigrants, filthy from exhausting journeys and infected with all manner of disease, to debark without proper supervision and examination?

Davis saw his life's work cut out for him. Within two years after his arrival in Chicago, he had given a series of lectures on hygiene and cleanliness, devised a permanent plan for the accommodation of the city's waste, taken the lead in organizing the first permanent hospital, inaugurated important changes in the policy and curriculum of the Rush Medical College, founded the Chicago Medical Society, and helped in the establishment of an association for the systematic relief of the poor. During the next fifty years Dr. Davis was to acquire a national reputation as a builder of medical and health institutions, a humanitarian reformer in medical and civic matters, and a sane and rational scientist.

His lectures on public health, delivered in the fall of 1850, were full of acute observations and much good sense. The city was only just recovering from the fearful effects of the great cholera epidemic of 1849, and the need for such advice was plain. Dr. Davis emphasized, above all, the need for fresh air. In an age in which disease was thought to be wafted on miasma-ridden breezes such counsel was all too uncommon. Chicago dwellings, he said, were crowded too closely together and the ventilation in these structures was notoriously poor. It was clear, he continued, why five women and children were sick for every male; daily confinement in improperly ventilated houses was doubtless the explanation. He addressed a number of comments to the mothers among his hearers; such practices as overdressing and binding babies in swaddling clothes were sharply criticized. Davis then turned to a consideration of the city water supply, importuning the authorities to establish a system which would ensure pure water for bathing, as well as for drinking. The sick poor, whether among the immigrant or native population, should be cared for, he urged, and he asked for support in his campaign to establish a hospital. The evils of intemperance, finally, came in for Davis' censure.[7]

Whatever proceeds derived from his public lectures were immediately re-invested in Davis' crusade for a hospital. In a letter to the Chicago *Daily Journal* in September, 1850, he reminded citizens that many ill persons were without home or friends in the city, and that the poor could not be properly cared for in their miserable hovels. Hospitals, it should

[7] *Gem of the Prairie*, Oct. 19, 1850. For Davis' outline of a drainage and sewerage system for Chicago, see *ibid.*, Oct. 12, 1850.

be remembered, were largely institutions for the shelter of the homeless sick at this stage in their development; no self-respecting person underwent hospitalization except by force of circumstance. Davis, in arguing his case, stressed the dual humanitarian and economic motive which his project would serve. Not only would it relieve a large amount of suffering, but a hospital would lessen the county tax for the sick poor, since such unfortunates might then be brought together in one place.[8] The undertaking met with success, largely through Davis' efforts, and the new institution, known as the Illinois General Hospital of the Lakes, was opened in October, 1850. The staff consisted of Davis, the noted surgeon Daniel Brainard, the talented and energetic John Evans, and Levi D. Boone, a physician and politician who subsequently became mayor of Chicago.[9] Nursing was in the care of students from Rush Medical College until the following year, when the Sisters of Mercy agreed to take over management of the hospital. As Mercy Hospital, the institution has compiled a long and notable record of accomplishment.[10]

Perhaps the most important organization tracing its origin to Dr. Davis' influence is the Northwestern University Medical School. This school owes its existence to a sharp difference in educational policy between Davis and Daniel Brainard, president of Rush Medical College. Brainard was a skillful surgeon who had come to Chicago in 1835. Restless and ambitious, his varied talents found outlets in politics, land speculation, surgery, and teaching. Competitive by nature, he desired to build Rush to a position of national prominence in medicine which meant, in the existing nature of things, that educational standards must be sacrificed in a free-for-all struggle to attract students. Thus it was that Davis, by accepting the Rush appointment, entered an atmosphere antagonistic to his deep-seated desire for reform. Brainard liked aggressive men of the calibre of Professor John Evans, whose infectious enthusiasm was a goad to more reticent teachers.

But Davis and Evans offer a study in contrasts. Both were energetic and forceful. Evans, however, accepted the commercial emphasis which a laissez-faire economy had imposed on medical education. To Davis, on

[8] Chicago *Daily Journal*, Sept. 26, 1850.

[9] *North-western Medical and Surgical Journal*, n. s. III (Sept., 1850), 263; *ibid.*, n. s. III (Mar., 1851), 492-93.

[10] See John B. Murphy, "The History of Mercy Hospital," Arthur H. Wilde (ed.), *Northwestern University; a History 1855-1905*, III (New York, 1905), 368-84, and the letter from Sister Mary Therese, Administrator of Mercy Hospital, to James P. Simonds, Chairman of the Chicago Medical Society Committee on History, Apr. 27, 1949, in the files of the Chicago Medical Society.

the other hand, financial success could never be the measure of an institution charged with educating men in the preservation of human life. While Davis wanted reform and higher standards, Evans worked for expansion and more students. Davis never wrote notes like the following to his colleagues:[11]

> Now Doctor I tell you what it is you must be up and doing—We must have 150 students this winter or break a trace and at least 25 of them must come from Michigan—now if you do not know where they are to be found it is time you were looking them up. I will take 25 & Fitch [another faculty member] 20 from this state [Indiana] and the other 75 must come from Ills. Wisconsin Iowa and elsewhere.

Davis was less interested in prospecting for students than in improving the training of men already in residence. He saw the chief needs of the medical schools as being enforcement of preliminary education requirements, lower tuition costs, and the substitution of a graded curriculum for the existing system of repeating each course twice during the two years of residence.[12] Why not divide all the courses taught in the colleges into two groups, the first consisting of the more basic subjects of anatomy, chemistry, materia medica, and physiology, to be followed the second year by the practical studies of surgery, obstetrics, and medicine?[13] In 1857 Professor Davis, seconded by several other faculty members, sought to introduce the graded course at Rush, but Brainard rejected the program on the grounds that higher standards would send the students elsewhere.[14] During Brainard's absence in Europe the following year, the faculty actually voted approval of the Davis proposals, but the president vetoed them again upon his return.[15] Finally, in 1859, Davis and four other

[11] John Evans to John McLean, June 21, 1847, MS., John Crerar Library.

[12] N. S. Davis, *History of Medical Education and Institutions in the United States* (Chicago, 1851), 168-69; N. S. Davis, "On the Intimate Relation of Medical Science to the Whole Field of Natural Sciences," Illinois State Medical Society, *Transactions*, 1853, 32-33. For an interesting study of the influence of Daniel Drake, famed pioneer western physician, on N. S. Davis, compare Daniel Drake, "Practical Essays on Medical Education and the Medical Profession in the United States," *Western Medical and Physical Journal*, V (June, 1831), 9-23, with N. S. Davis, *Inaugural Address Delivered at the Opening of the Medical Department of Lind University* (Chicago, 1850). This address was also printed in the first volume of the *Chicago Medical Examiner*.

[13] *North-western Medical and Surgical Journal*, n. s. V (Mar., 1856), 138-45.

[14] H. A. Johnson, *History of the Chicago Medical College; an Introductory Lecture to the College Session of 1870-71* (Chicago, 1870), 8-9. Johnson was one of the professors who left Rush with Davis.

[15] N. S. Davis III, "Nathan Smith Davis," M. Fishbein (ed.), *History of the American Medical Association*, 10.

professors resigned from Rush to form the nucleus of a new medical school, which attached itself immediately to an ephemeral institution known as Lind University.[16] When Lind failed, in 1862, Davis' school continued as the Chicago Medical College. An affiliation was reached with Northwestern University in 1869, but the final incorporation of the medical school into the university did not occur until 1891.[17]

Davis' ideas were put into operation immediately at the new school. The lecture term was lengthened to five months; fewer lectures and more examinations were provided for; and, most important of all, the curriculum was divided into a junior and senior division.[18] The Chicago Medical College was the first school in the country to thus lengthen the term of study and inaugurate the graded course, though Charles W. Eliot of Harvard later wrote that he had "mistakenly thought that the Harvard Medical School was the pioneer in these respects."[19]

The Rush professors continued to attack Davis and his ideas, while the personal feud between him and Brainard grew increasingly sharp in the years following the formal split. The *Chicago Medical Journal*, which was the Rush organ, usually referred to Davis' college as the "Reform School" and to Davis as "the Apostle." A typical item accused the "model great medical reform school" of being located in the "cockloft of a warehouse"—a reference to the inadequate character of classroom facilities at the college.[20] After Brainard's death, in 1866, Davis proposed the reunion of the two institutions on the basis of a compromise between the Rush curriculum and the graded, lengthened course of his own school. His offer was apparently never seriously entertained since the Rush journal continued adamant in its opposition to reform ideas.[21]

[16] Northwestern University Medical School, Minutes of Faculty Meetings 1859-1881, pp. 1-6, MS., Church Library, Northwestern University Medical School; Samuel J. Jones, "History of Northwestern University Medical School," H. G. Cutler (ed.), *Medical and Dental Colleges of the West; Chicago* (Chicago, 1896), 159-60.

[17] S. J. Jones, "History of Northwestern University Medical School," H. G. Cutler (ed.), *Medical and Dental Colleges of the West*, 163-64, 167.

[18] N. S. Davis, *Inaugural Address*, 11-12; *First Annual Announcement of the Medical Department of Lind University* (Chicago, 1859), 5-11.

[19] Elizabeth Carr, "Northwestern University Medical School and the Graded Curriculum," *Quarterly Bulletin of Northwestern University Medical School*, XV (1941), 56-61; Charles W. Eliot to N. S. Davis, Jr., Aug. 24, 1896, MS., Church Library, Northwestern University Medical School.

[20] *Chicago Medical Journal*, n. s. IV (Sept., 1861), 539-40. For Brainard's fullest statement regarding the Chicago Medical College, see *ibid.*, n. s. II (June, 1859), 381-85.

[21] N. S. Davis, Basis of Union for the Rush and Chicago Medical Colleges, Dec. 1, 1866, MS., Church Library, Northwestern University Medical School.

Despite the fact that Rush was well established and continued to offer degrees after considerably less study, the Chicago Medical College began slowly to expand its enrollment and facilities. From thirty-three students the first year, the number of matriculants rose to eighty-one in 1863, a figure larger than at Yale or Dartmouth.[22] In 1870 a new and more satisfactory building was erected, only to be devoured in the flames of the Great Fire the following year.[23] The progress of Davis' plans was reflected in the creation of new departments of gynecology, nervous and mental diseases, histology, laryngology and rhinology, pediatrics, physical diagnosis and dermatology in the two decades following occupation of the new building.[24] Laboratory work was especially encouraged; it was one professor's belief that much of the expense for instruction and supplies in this department was borne by Davis personally.[25] In 1884 Davis suggested that class lectures be supplemented in all subjects with recitations to encourage the students "to study more attentively and systematically."[26] Two years later an event of great importance took place when Professor Bayard Holmes began the first formal instruction in bacteriology in any Chicago medical school.[27] By 1891, when the school became an integral part of Northwestern University, Davis' work was complete. Though he remained active for a few more years, his great contributions to medical education were now in the past.

Of all his varied activities, none was dearer to Davis' heart than professional organization. He had first attracted national attention by his efforts on behalf of the American Medical Association. In Chicago he found a divided and captious profession; since 1836 no effort had been made to unite the city's doctors in any type of medical fraternity.[28]

[22] N. S. Davis, *Lecture Introductory to the Fifth Annual Course of Instruction in the Chicago Medical College* (Chicago, 1863), 9.

[23] Chicago *Tribune*, July 15, 1870.

[24] Nathan S. Davis, Jr., "The Later History of the Medical School," A. H. Wilde (ed.), *Northwestern University*, III, 320.

[25] John H. Long, "The Laboratories, the Museum and Library," A. H. Wilde (ed.), *Northwestern University*, III, 338.

[26] Northwestern University Medical School, Record of Proceedings of Faculty Meetings . . . from April 28th, 1884 to Sept. 26, 1891 Inclusive, pp. 20-21, MS., Church Library, Northwestern University Medical School.

[27] For a comparison of Chicago's development in this respect with the rest of the nation, see David H. Bergey, "Early Instruction in Bacteriology in the United States," *Annals of Medical History*, I (1917), 426-27.

[28] Chicago *American*, Oct. 1, 1836; Lucius H. Zeuch, "Early Medical Legislation and Organization in the Illinois Country," *Bulletin of the Society of Medical History of Chicago*, IV (July, 1930), 208-09.

Neither was there any state or regional society which Chicago doctors might attend. There were reasons, of course, why medical organization tended to lag. The pioneering physicians of the 1840's and 1850's were notoriously busy; transportation was excruciatingly slow and unreliable; many medical men divided their time between medicine and agriculture, or real estate, and thought the latter the more important. Few doctors, moreover, could afford the expense of an extended trip or the financial loss of a non-productive period. Professional income, Davis later recalled, "scarcely equal[led] those resulting from almost any of the mechanic arts." [29] It took real interest and persistence for Davis to undertake a journey like the following in order to attend a meeting in Jacksonville, Illinois: [30]

> I had a journey by stage of one day and a good part of a night, then a ride on the Illinois River, then another piece of a ride on the construction train of a railroad on top of a load of ties, part way to Jacksonville, and I think we came into the city on a stage coach.

Within a year after his arrival in the West, young Dr. Davis took part in the organization of both the Chicago and Illinois Medical Societies. In the case of the former, his role was virtually that of founder. The preliminary meeting of the Chicago Medical Society was held in April, 1850, amid gloomy prophecies that the city's highly individualistic physicians could never join together in any effective association. These predictions were partially fulfilled when the influential Dr. Brainard withdrew after the first meeting, ostensibly because of the pressure of work, but probably because of differences between him and Davis. The weaknesses of the new society were readily apparent: it lacked sufficient support from the profession and it lacked power to enforce standards of professional conduct. When a certain Dr. J. E. McGin was charged with using "very abusive and unprofessional language" to describe the society's first president, he promptly told the investigating committee that "the society might go to h-ll." [31]

A re-organization was effected by Dr. Davis, in 1852, in a successful

[29] "Semi-Centennial of the Illinois State Medical Society," *Medical Standard*, XXIII (June, 1900), 356-57.

[30] "Archives Era," *Illinois Medical Journal*, LXXVII (May, 1940), 391.

[31] The only sources of information regarding the first two years of the Chicago Medical Society are two articles by N. S. Davis: "A Brief History of the Chicago Medical Society from Its Origins to the End of Its First Half Century of Progress," *Chicago Medical Recorder*, XI (1901), 199-204; "Brief Reminiscences of the Origin and Growth of the Chicago Medical Society," *ibid.*, XII (1902), 365-69.

effort to resuscitate the moribund society. Rules and regulations were simplified, the number necessary for a quorum was reduced and the name was changed to Cook County Medical Society to attract county physicians. The name of the association was changed back to Chicago Medical Society in 1858 when the lone non-Chicagoan among the members died. In 1903 it again became a county organization but the Chicago title was retained.[32]

During Davis' lifetime the scientific papers read before the group dealt chiefly with the epidemic diseases which periodically swept over the city. Davis himself took a vigorous part in discussions of typhoid, cholera, smallpox, and diphtheria. The society took an interest also in problems of hygiene and sanitation which lay on the borderline between civic and medical responsibility. As early as 1854 Davis was appointed chairman of a committee charged with investigating sanitary conditions in the city. The group made several reports, the final one of which concluded that the "Sanitary condition . . . is about as bad as Nature and Art can make it."[33] The condition of the poor received the sympathetic consideration of Davis and his colleagues. A Committee on the Sick Poor, established in 1864, recommended that the city support dispensaries and visiting physicians for the indigent.[34] Dr. Davis, unlike many of his contemporaries, early recognized the importance of social factors in the genesis of disease. In a paper delivered in 1870 he attributed the causes of relapsing fever "to the wants of poverty and destitution in crowded and badly ventilated, uncleanly habitations."[35]

Dr. Davis was a charter member of several non-medical scientific societies. The close relationship between the medical and natural sciences, for example, was responsible for his role in organizing the Chicago Academy of Natural Sciences (now the Chicago Academy of Sciences) in 1856. He was not the only physician involved in the founding; of the dozen or so original members, five were medical men. Representatives of the profession, indeed, dominated the first meetings of the body. All save one of the five most important elective offices were tendered to physicians. Davis was chosen one of the two vice-presidents.[36]

[32] Chicago Medical Society, Minutes, Apr. 5, 1852, Aug. 3, 1858, Feb. 4, 1903, MSS., Chicago Historical Society.

[33] *Ibid.*, May 13, 1856.

[34] *Ibid.*, Oct. 28, 1864.

[35] *Ibid.*, Feb. 28, 1870.

[36] William K. Higley, *Historical Sketch of the Academy* (Chicago Academy of Sciences, Special Publication No. 1, 1902), 5.

The irrepressible Davis was instrumental in the founding and administration of numerous other educational, scientific, and reform projects. Five years after his coming to Rush, he assumed the editorship of the only medical journal published in the city. When he left Rush to found his own school, he started a second monthly, the *Chicago Medical Examiner*. At various other times he edited such differing publications as the *Literary Review*, the *Eclectic Journal of Education*, and the *American Medical Temperance Quarterly*.[37] In 1883 he became the first editor of the *Journal of the American Medical Association*.[38] His interest in medical literature was well known. When, therefore, a Medical Library Association was founded in Chicago, what more logical person than Dr. Davis to direct the quest for a public medical library?[39] He had already dickered with the noted eastern physician and historian, Joseph M. Toner, for his large personal library.[40] Partially as a result of Davis' work, a medical department was finally established at the Newberry Library in 1890.[41] In addition to these activities, Dr. Davis found time to assist in the founding of the Illinois Microscopical Society and the Washingtonian Home for alcoholics, as well as serve for many years on the Board of Trustees of Northwestern University.

In medical matters Davis was commonly cautious and often suspicious of unproven innovations. He was a bitter foe of homeopathy and scornful of those who would treat it as simply another theory of etiology; he was too thoroughly imbued with the positivism of nineteenth-century clinical medicine to accept such reasoning. His mind was empirical and practical; he was skeptical of theories, especially those which did not conform entirely to the brute facts of clinical experience. He was, in a word, the prototype

[37] N. S. Davis, "A Brief History of Medical Journalism in Chicago to the End of the Nineteenth Century," *Clinical Review*, XVI (Sept., 1902), 468-71; Lucius H. Zeuch (ed.), *History of Medical Practice in Illinois*: Vol. I; *Preceding 1850* (Chicago, 1927), 225-28. It is interesting to note that Davis' report on medical literature to the American Medical Association, in 1853, followed the distinguished precedent set by Oliver Wendell Holmes. Davis found very little American medical writing which served any "other purpose than to fill a certain space in the Journal, and afford the author the pleasure of seeing his name in print." "Report of the Committee on Medical Literature," American Medical Association, *Transactions*, VI (1853), 106.

[38] M. Fishbein, *History of the American Medical Association*, 109.

[39] Chicago Medical Library Association, Papers, Chicago Historical Society.

[40] J. M. Toner to N. S. Davis, Dec. 27, 1878, Jan. 4, 1879, MSS., Church Library, Northwestern University Medical School.

[41] Board of Trustees, Newberry Library, to N. S. Davis, Dec. 28, 1889, MS., Chicago Historical Society.

of the skeptical, questioning, testing, measuring man of science of the nineteenth century.

Hence it was that Davis found it difficult to accept the germ theory of disease which burst upon the medical world in the 1870's and 1880's. Like so many men of his generation, he was able to adopt antisepsis because of the practical, measurable results which it brought, but he could not submit to a hypothesis which seemed to run counter to all verifiable experience. No aspect of Davis' career has received so much attention as this fight against the germ theory; yet no part of his life has been so thoroughly misunderstood. His contemporaries judged him in the light of their own progressive beliefs, while many of his students remembered him as a kindly, if misguided, bigot who clung to outmoded doctrines when confronted by revolutionary change. Nothing could be more unfair than such a characterization. For Davis in truth was no blind opponent of the bacterial theory; he opposed only certain aspects of the doctrines of Koch and Pasteur and in some respects his warnings make sense to the twentieth-century investigator.

As early as 1876 Davis signalized his willingness to admit the possibility of a germ cause for syphilis and the eruptive fevers, though he was skeptical of the idea that specific germs were inevitably associated with the origin of certain diseases. In any case, he reasoned, etiology did not constitute a sound basis for the diagnosis of illness because of the difficulty of deciding at the bedside whether a particular germ were or were not present.[42] In the years following, Davis asserted that the germ theory, like other theories of the past, could never explain the whole etiology of disease. Typhoid fever, for example, originated seemingly *de novo* in isolated surroundings with no contact with other typhoid sufferers. There was "no direct or positive evidence," he maintained, "that this much talked of 'typhoid *germ*' has any existence except in the human imagination, for no one has succeeded in isolating, and satisfactorily identifying it." Scientists, he warned, should adhere "to well-ascertained facts, and avoid hasty and positive conclusions until all the facts have been ascertained."[43]

Davis was wrong about typhoid fever but he was right in believing that the germ hypothesis would not provide an explanation for the origin

[42] N. S. Davis, "Does Etiology Constitute a Proper Basis for the Classification and Diagnosis of Diseases?" *American Practitioner*, XIII (Louisville, 1876), 343-52.

[43] N. S. Davis, *Lectures on the Principles and Practice of Medicine* (Chicago, 1884) 77-85.

of all illness. While others joined the enthusiastic hunt for new microbes, he continued to emphasize the importance of bodily resistance—of the *vis medicatrix naturae*—in selecting the victims of disease. If germs were the sole cause of illness, as the champions of the new doctrine were proclaiming, then why did not every body invaded by bacteria contract disease? In the absence of a scientific explanation of the phenomenon of natural immunity, his question was a pertinent one.[44]

As the role of pathogenic bacteria became more firmly established during the 1880's, Davis shifted his attack. He now admitted the importance of micro-organisms in the genesis of disease, but emphasized more strongly the necessity of recognizing the role of social and constitutional factors.[45] In 1894 he asked "whether, at the present time, the attention of Sanitarians is not given too exclusively to the *specific* causes, more especially of a bacteriological character and their ptomaines; while much more might be accomplished by giving more attention to the predisposing influences both as regards those pertaining to the individual and to his surroundings." [46] His cautious nature rebelled at the sight of medical men rushing off to pursue studies in bacteriology, while neglecting the cumulative advances of a century in the fields of social and preventive medicine. In the case of tuberculosis, for example, he argued that Koch's discovery of the bacillus had set back, rather than advanced the management of tuberculous patients; for the discovery had resulted in an over-attention to the search for a means of destroying the bacillus, while previous knowledge of predisposing and prophylactic influences was forgotten.[47]

One might conclude that Davis' approach to the new theory was con-

[44] *Ibid.*, 32.

[45] See, for example, N. S. Davis, *The Basis of Scientific Medicine and the Proper Methods of Investigation* (Reprinted from the *Journal of the American Medical Association*, Jan. 24, 1891), 15-16; "On the Influence of the Ship Canal, Now Being Constructed . . . on the Sanitary Condition of Chicago and the State of Illinois," Illinois State Medical Society, *Transactions*, 1893, 467; *The History, Present Status, and Future Progress of Practical Medicine* (Reprinted from the *Chicago Clinical Review*, Oct., 1893), 9.

[46] N. S. Davis to Charles N. Shepard, May 28, 1894, MS., Church Library, Northwestern University Medical School.

[47] N. S. Davis, *What Are the Most Efficient and Practical Means for Limiting the Prevalence and Fatality of Pulmonary Tuberculosis?* (Chicago, 1899), 4-5. (In N. S. Davis, Collected Papers, 1849-1904, John Crerar Library.)

servative, but open-minded. He accepted scientific demonstrations of the causative role of micro-organisms in illness, but warned his colleagues that their importance might be over-emphasized. The soil in which bacteria were planted, he reasoned, could be as important as the bacteria themselves. He anticipated, in a word, the twentieth-century reaction to the zealous over-application of the doctrines of specificity and solidism to all pathological conditions. Ridicule of his views came from those who shared the enthusiastic belief of the last two decades of the nineteenth century that the problem of the origin of all disease was about to be solved for all time. It should not be forgotten that at the time Davis wrote, bacteria-proof houses were in the planning stage and the end of therapeutic medicine was cheerfully predicted. The microbic origin of cancer was not only widely believed but many thought the specific organism had been found.[48] In many respects, then, Davis was in the van rather than the rear-guard of medical science on this issue. His rich experience and healthy skepticism preserved him from the excesses of his colleagues and informed him that all empirical medical knowledge was not to be invalidated by a single discovery, or even by a series of them.[49]

Much has been said about Davis' contributions to medicine and public health and the building of medical institutions. But perhaps his most important influence on his contemporaries lay in the impact of his personality and is hence not subject to recapture in the journals or documents. He was possessed of tremendous sincerity, an unwavering integrity and a powerful sense of justice; he became the stern conscience of his generation of medical men. Though a strong believer in the individualistic creed of nineteenth-century America, he never favored progress at the expense of the underdog. In public and private decisions, conscience rather than self-interest was more likely to be his guide. He knew the sting of ridicule felt by the champion of unpopular causes; he bore manfully the stigma which attached to followers of the Copperhead cause during the Civil War. His sympathy for the poor and the underprivileged was warm and genuine. Out of his own limited income (he never raised his standard fee of one dollar per office call) he gave generously to humanitarian causes, and he gave freely of his time to charity work.

[48] See A. P. Ohlmacher, "The Parasite of Carcinoma," *The Scalpel*, I (Aug., 1892), 35-40. This publication was a student magazine at the College of Physicians and Surgeons of Chicago.

[49] For Davis' last and most mature statement, see his *History of Medicine* (Chicago, 1903), 160-72.

In appearance he was slight in stature and physique, though erect and muscular in development. He looked to be literally a doctor of the Old School with his shaggy hair, side whiskers, and swallow-tail coat which he wore until his death in 1904. His manner was positive and forceful and apt to be interpreted as gruff by those who did not know him well. But he was unfailingly kind and helpful to those who sought his aid; his correspondence is full of letters from students and friends who acknowledged favors, loans, or counsel. He wrote long letters of advice and encouragement to young Chicago doctors in Europe. Arthur R. Edwards, the recipient of one of these letters, remarked after unburdening himself to Davis of all his youthful ambitions that he would hesitate to confide in "anyone not so sympathetic and helpful." [50] He received numerous requests from persons all over the country for medical advice, as though a literary diagnosis or favorite prescription from Davis were better than the services of any local doctor. Students found in him an especial warmth and interest, whether they were studying medicine or Greek, in his school or in any other. One of the many letters which gladdened Davis' last years came from a Rockford, Iowa, minister, whom he had assisted in getting through Garrett Biblical Institute. This young man remembered with feeling that Davis had told him "that if I was not able to return the money . . . perhaps I could assist some other poor boy in securing an education." [51]

Not only Chicago, but the American medical profession owes a large debt to Nathan S. Davis. He represented the highest type of medical man produced in nineteenth-century America. In him were lodged a unique combination of energy, integrity, and warm humanity. His example inspired many doctors to higher ideals of conduct and achievement. "Our knowledge," recalled one former student, "was acquired from all of our professors, but our inspiration came from him." [52] At a great testimonial banquet tendered him in 1901, this remarkable physician received the thanks of his city and his profession. Surrounded by colleagues from all over the country come to pay him tribute, the eighty-four year old Davis related the "single principle of action" which had guided

[50] Arthur R. Edwards to N. S. Davis, July 9, 1893, MS., Church Library, Northwestern University Medical School.

[51] W. B. Davis to N. S. Davis, Feb. 18, 1896, MS., Church Library, Northwestern University Medical School.

[52] Henry T. Byford, "Davis, Nathan Smith," Howard A. Kelly and Walter L. Burrage (eds.), *American Medical Biographies* (Baltimore, 1920), 293.

his life. His inspiration drew breath from a promise made as a young boy to his dying mother:[53]

> She said to me that she wished me to be a good boy, to learn to worship God, and to do good to my fellow-men. I promised her I would. Of course, I did not realize the importance or bearing of that promise at that early period of life; but an impression was made upon my mind, and from that day to this the rule of my life has been, that whatever comes up that seems to be important and will improve my fellow-men, my impulse is to do what I can to help it along.

In this mild understatement was summarized the creed of a great physician and a Christian gentleman.

[53] *Testimonial Banquet Given in Honor of N. S. Davis* (Chicago, 1901), 5-6.

THE HISTORY OF LAMINECTOMY PRIOR TO 1866*

JAMES W. MARKHAM

In reviewing the development of spinal cord surgery, it became apparent to this writer that certain inaccuracies exist in the literature relevant to the early history of laminectomy. It seemed desirable to make known these discrepancies in order that the historical basis of spinal surgery may be further clarified.

Many writers (14, 16, 19, 20) on this subject have been in agreement that Cline of London, in 1814, performed the first formal laminectomy and have accredited Edwards in 1838 with the first successful laminectomy (34, 38, 40, 60). However, it is evident that at least one successful operation of this kind antedated Edwards' case, viz. that of Alban Smith of Kentucky in 1828, and to the latter credit should be given for this accomplishment.

The development of spinal surgery prior to the nineteenth century has been described elsewhere by this writer (41). Surprisingly little was known of spinal cord physiology before the nineteenth century, while operative treatment of surgical conditions affecting the spinal cord had made practically no advance since the seventh century. Operative treatment of traumatic lesions of the spine with neurological complications had been recommended, first by Paul of Aegina in the seventh century, and later by Albucasis in the twelfth century, Ambroise Paré (1510-1590), Vigier (16th century), Fabricius Hildanus (1560-1624), James in 1745, Géraud in 1745 and Heister (1683-1758). But not more than three surgeons appear to have made actual surgical attacks upon the injured spine—Ambroise Paré (47), Géraud (22) and Louis (39); moreover, from the descriptions given of these attempts one cannot be certain that injured laminae were removed and the spinal cord exposed. It would appear that at best in these three instances, the surgical attempt was confined to exploration of the depths of open wounds with extraction of loose fragments—or at most simple debridement of open wounds.

There is little doubt then that Cline's operation in 1814 was the first which met the requirements of the term laminectomy. Although he did not report his case, the details were given by Hayward (31) in 1815.

* From the Division of Neurological Surgery, The Johns Hopkins University School of Medicine, Baltimore, Maryland.

The patient was a young man of twenty-six years who had fallen from a housetop, sustaining a paraplegia below the waist. The spinous processes of the seventh, eight and ninth thoracic vertebrae were depressed. On June 16, 1814, the day following injury, Cline made a vertical incision four to five inches in length directly over the injured spines. He dissected the paravertebral muscles free and cut away the seventh and eighth thoracic spinous processes with bone forceps but did not use the mallet and chisel. It was noted the vertebrae were dislocated but the attempt to establish realignment was unsuccessful, and the patient died two days later.

The second case which has been accepted as an instance of laminectomy was that of Wickham (67) in 1819, but the details of operation are not known since Wickham did not report the case. What little is known of the operation was given by Tyrrell (67), who stated the case was one of fracture of the arch of the seventh cervical vertebra. The operation was done eight days after injury, and although the patient stated that postoperatively the sensation in his arms was improved, death followed the next day.

The same may be said of Oldknow's operation in the same year (44). Again Tyrrell (67) is the only authentic source of information regarding this case and the details are scant. The patient had a fracture of the transverse process of the seventh cervical vertebra. Operation was done six days after injury and the patient died one day later from extradural hemorrhage.

Even less, in fact not even the year of operation, is known of the laminectomy reported to have been done by Attenburrow (1).

Tyrrell's attempt, in 1822, was unsuccessful (63). The patient was a young man twenty-five years of age who had a fracture of the twelfth thoracic vertebral arch. Sensation was lost below Poupart's ligament on both sides, and movement of the legs was impossible. Tyrrell made a vertical median incision and removed the laminae with a Hey saw. The dura mater was not opened but the cord was seen to pulsate and the wound was closed. Although sensation returned in the legs afterward, meningitis developed and the patient died on the thirteenth post-operative day.

Barton's case, in 1824, represents the first laminectomy performed in America. The case was reported by Godman the following year (2). The patient was a sailor who fell from a mast. Operation was performed by Barton twelve days after injury because of paraplegia. An incision, eight inches in length, was made over the thoracic spines. The spinous

process and the arch of the seventh thoracic vertebra were found fractured and depressed, while the bodies of the seventh and eighth thoracic vertebrae were dislocated. Barton removed the compressing fragments and managed to realign the vertebrae. The wound was covered with lint. Although some sensation returned to the legs, the patient had chills forty-eight hours later and died sixty hours postoperatively of a massive hemothorax.

Tyrrell (64) operated on another case of fracture dislocation in 1827 but again the outcome was fatal.

In 1827 Holscher (33) of Hannover, performed a laminectomy for fracture dislocation of the eleventh and twelfth thoracic vertebrae thirteen days after injury. Although there was a temporary improvement following operation, the patient's course turned to the worse, complications developed, and he died approximately three months afterward. It should be noted also that Holscher did not report this case until 1839, i. e. twelve years later; and one wonders whether the date of operation given as 1827 was not a misprint for 1837.

In any case, there seems little doubt about the fact that Alban Smith's patient was the first to survive laminectomy for a prolonged period and to benefit from the operation; moreover, Smith reported his case within two years, or ten years before Holscher.

The account of Smith's operation appeared in the *North American Medical and Surgical Journal* in July, 1829 (55), and began as follows:

> About two years since, I was called a distance of about twelve miles, to a place called Pleasant Hill, the residence of a community of the people commonly called Shakers, to see a young gentleman, a member of the society, who had fallen from a horse, and had become paralysed in all the extremities with the exception of the muscles above the elbow joint.

Smith was unwilling to attempt surgical attack upon the injury at that time, since he was of the opinion that the spine was dislocated as well as fractured, and this argued for a hopeless prognosis. He was, therefore, much interested when, two years later, relatives of the patient approached him and pleaded for an operation. After some consideration, Smith resolved to undertake an exploration of the injured spine. His description was as follows:

> I made an incision, about five or six inches in length, along the spinal ridge, and then two deep incisions, traversely to, and at each extremity of the former, and three inches and a quarter in length, all down to the bone. I then dissected all the

muscles and ligaments as far as the heads of the transverse processes, and scraped the bone clean. . . .

I next took a small Hey's saw, and made an incision on both sides, as near the base of the transverse processes as I could, as deep as I could with safety to the spinal marrow and the nerves going off at the sides, and long enough to cut through the bone above, about half way, which was the third dorsal vertebra.

[The article went on to describe the removal of the spinous processes of the second and fourth dorsal vertebrae and the lamina of the fourth, which was the site of the injury] . . . I now brought the parts together, putting a tent at the bottom; and put him to bed. He had a chill, succeeded by a fever, and some bilious symptoms, which were carried off by a dose of calomel.

Unfortunately, Smith's notes on the post-operative progress of his patient were sketchy. He apparently did not see the patient again until a week later. At that time, he noted " the ulcers in his gluteal muscles . . . were healing up; and he had some additional sensation in his hands. . . . His latest progress note, dated May 7, 1829, contained no more information than ". . . feeling has now got down into his thighs: You never saw a creature so overjoyed in your life." It is regrettable that more detailed notes on the results of the operation were not given. However. it was revealed by Gross, twenty-four years later, that the patient lived for approximately five years after operation (26).

Some uncertainty arises as to the exact dates of injury and of operation. Gross (27) gave the date of injury as March 31, 1827, which would not seem to conflict with Smith's article (which began with " About two years since . . ."). Gross stated that the operation took place on October 5, 1827, but this seems quite different from the content of Smith's article. Writing in 1829, Smith made it clear that there was an interval of about two years between his first visit and the operation. Since Smith's latest follow-up letter was written in May, 1829, and there was at least one other progress note between this date and the original draft of the article, it is probable that the latter, obviously composed not long after the operation, was written early in 1829. This would tend to place the date of operation early in 1829 or late in 1828. The latter date seeems the more likely.

If the operation had been performed in October, 1827, as stated by Gross, it would not appear likely that Smith, writing in 1829, would have been still so optimistic. Gross based his dates on a letter written to him on September 6, 1852, by a neighbor who had known Smith's patient. Since this letter was written twenty-five years after the accident, its accuracy as to exact dates could well be questioned. However, because

of this letter, it can be ascertained that the patient survived the operation approximately five years and that some degree of sensation returned to the lower extremities, albeit he remained an invalid. The cause of death was not clearly revealed.

It appears that Smith's case is the first successful laminectomy reported in the literature. A review of numerous historical works on surgery (4, 7, 9, 11, 12, 15, 30, 49) and surgical texts (3, 16, 28, 46, 48, 54, 56, 57, 65) of the nineteenth century reveals that up to the year 1840, there were but twelve instances in which laminectomy had been performed. The indication in every case was either fracture, dislocation, or both, but the only ones which could be regarded as successful were those of Alban Smith and of Edwards.

1.	Cline (31)	1814
2.	Wickham (67)	1819
3.	Oldknow (44)	1819
4.	Attenburrow (1)	1819–1820?
5.	Tyrrell (63)	1822
6.	Barton (2)	1824
7.	Tyrrell (64)	1827
8.	Holscher (33)	1827
9.	Smith (55)	1828
10.	Rogers (52)	1834
11.	Heine (32)	1835–1850?
12.	Edwards (18)	1838

The basis upon which Edwards has been regarded as the first to operate successfully for spinal fracture was a statement by Sir Charles Bell in 1838: "An operation of this kind has been done of late by Edwards, of Caerphilly, South Wales. The posterior arch of a lumbar vertebra was raised and the symptoms of compression relieved. I am informed the patient did well" (18).

Bell said this was the only instance of which he knew in which the result was satisfactory, or in which the patient survived. But Edwards did not report the case himself, and from the scant description given by Bell one cannot be certain that the operation consisted of anything more than the elevation of a depressed lamina. Lloyd (38) cited McDonnell (40) as saying that this patient recovered perfectly from his paraplegia, returned to work, and fathered children; but this was not confirmed on reviewing McDonnell's writings or elsewhere in the literature. Thus it

seems that the basis for regarding Edwards as the first to perform a successful laminectomy was a not very well founded report.

From 1839 up to the year 1866 there were seventeen laminectomies performed. Although some authors have included South and Boyer, this did not seem justified upon review of their works (8, 59).

13.	Laugier (37)	1840	
14.	Potter (50)	1844	
15.	Walker (66)	1845	
16.	Mayer (43)	1846	
17.	Roux (53)	1850?	
18.	Blair (6)	1852	
19.	Blackman (5)	1854	
20.	Blackman (5)	1854	
21.	Jones (36)	1856	
22.	Hutchinson (34)	1857	
23.	Smith, S. (58)	1857	
24.	Potter (51)	1859	
25.	Potter (51)	1860	
26.	Potter (51)	1862	(same patient as no. 24)
27.	McDonnell (40)	1865	
28.	Gordon (25)	1865	
29.	Tillaux (61)	1865	

Thus it may be stated that up to the year 1867 only twenty-nine laminectomies were reported in the literature; of these, all but one were performed for traumatic conditions, except that of Roux which was incidental to removal of a sarcoma of the spine (53). Of the twenty-nine cases, only six could be regarded as successful, viz. those of Alban Smith, 1828; Edwards, 1838; Blackman, 1854; Potter, 1859; Potter, 1862; and Gordon, 1865. The case of Blair (6), in 1852, cannot be regarded as a successful case, since no clinical information was given and since all that was mentioned about it was that "a successful result was obtained"—which leaves a great deal of doubt as to what was meant by this phrase.

Although Smith's article was not unknown to many writers on the subject of spinal surgery prior to 1900, it appears that the importance of his report was not duly appreciated. It is unfortunate that more interest was not taken in this case, and that Smith was not given due credit for his notable accomplishment. Medical historians of the nineteenth century (17, 29, 45, 62, 68) and contemporary reviews (9, 10, 13, 14, 21, 30) of surgery either did not mention Smith's contribution or failed to recognize its importance.

This was a remarkable example of truly pioneer surgery, performed in the frontier days of Kentucky, far from the medical centers of the east and Europe, and with little precedent. It seems fitting that this should have been done by a man who had served a preceptorship under the great Ephraim McDowell and is believed to have been present at his celebrated ovariotomy in 1809.[1]

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[1] Smith himself performed the third successful ovariotomy in the United States in 1823 (35). He was one of the earliest lithotomists in the U. S. Later, after moving to New York, he confined his practice to genito-urinary surgery and wrote two treatises on this subject (23, 24). His name was changed to Alban Goldsmith by an act of the New York Legislature. A biographical sketch has been given elsewhere (42).

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MEDICO-HISTORICAL NEWS AND ACTIVITIES

CORRESPONDENCE AND REPORTS

Founding of an International Academy of the History of Pharmacy
(Académie Internationale d'Histoire de la Pharmacie)

Early this year about fifty prominent scientific representatives of the history of pharmacy in about twenty countries, mostly university teachers, agreed on the founding of an International Academy of the History of Pharmacy. In their opinion such an institution will be of great importance for the scientific pursuit of the history of pharmacy, hence for pharmacy at large.

The "Academy" has its seat in The Hague (Holland), the place that has become traditionally connected with the idea of peaceful cooperation of all peoples of the world. The official language is French, with English, German and Spanish as permissible substitutes or supplements.

The "Academy" is, in accordance with the common definition of this term, neither a teaching institution nor a private association of people joining on their own volition. It stands quite by itself, completely independent from all private organizations of whatever kind, and its particular tasks exclude every competition with them. The statutes of the "Academy" state as its main purpose "to stimulate international cooperation in the history of pharmacy and, in particular, to act as a pharmaceutical-historical Upper House." That includes the pursuit of projects of international importance recognized as such or initiated by the "Academy" and asking for cooperative work by experts of the caliber as warranted by membership in the "Academy." Special emphasis is laid on the "Upper House" functions of the "Academy" which undoubtedly will exert a definite influence on the place given to the history of pharmacy as an academic branch of science in the higher institutions of learning all over the world.

A further aim is the establishment of a library serving the students of the history of pharmacy and of respective archives.

Following the customary rules of academies in other fields, the statutes of the International Academy of the History of Pharmacy provide for a limited number of members, new members to be elected by the old ones in case of a vacancy by death or resignation whereby suggestions may be made from outside as well as inside of the "Academy." This method secures a membership comprising only men of generally recognized importance, simultaneously making election to the "Academy" an honor of international meaning.

The official founding date is the thirteenth of June, 1952. This date was chosen in honor of George Urdang, Professor of the History of Pharmacy at the University of Wisconsin, U. S. A., who is celebrating his seventieth birthday on this day. For the same reason the statutes bestow on Urdang the first term of office as President of the "Academy" (otherwise rotating every three years) for the time up to December 31, 1955. Simultaneously he is honored with the choice and

appointment of the first members of the "Academy" which has to be taken care of by December 31, 1952.

The start of the "Academy" was made possible financially by donations of numerous interested individuals and pharmaceutical organizations. For the future the income is supposed to come from contributions of the countries represented in the membership. It is hoped, furthermore, that the "Academy" will find in its sphere the support which its sister organizations in other fields are enjoying.

The address of the "Academy" is: Alexanderstraat 11, The Hague, Holland. The office of Secretary is held by Dr. P. H. Brans, Heemraadssingel 122, Rotterdam-C., Holland.

The Chairman of the Founding Committee, George Edmund Dann, Lecturer in the History of Pharmacy, University of Kiel, Kiel (Germany).

ANNOUNCEMENTS

AMERICAN ASSOCIATION OF THE HISTORY OF MEDICINE

TWENTY-SIXTH ANNUAL MEETING: COLUMBUS, OHIO, APRIL 10, 11, AND 12, 1953.

Dr. John F. Fulton, President of the American Association of the History of Medicine, has accepted an invitation of the Ohio Academy of Medical History to hold the next annual meeting of the Association in Columbus, Ohio, in connection with the Sesqui-Centennial Celebration of the State. The meeting will take place April 10, 11, and 12, 1953, with the dinner session scheduled for the evening of Saturday, April 11. Headquarters: Fort Hayes Hotel. Professor Linden F. Edwards, Department of Anatomy, Hamilton Hall, Ohio State University, Columbus, 10, Ohio, is chairman of the program committee.

THE JOHNS HOPKINS UNIVERSITY
INSTITUTE OF THE HISTORY OF MEDICINE

Fellowships in the History of Medicine and of Science

The Institute of the History of Medicine, of The Johns Hopkins University, announces the continuation of its fellowships in the history of medicine and of the natural sciences. One purpose of these awards, which provide from $2,000 to $3,000 in each case, is to supplement the training of young scholars who may have become interested in the history or sociology of science. Those who hold the M. D. or the Ph. D., or who have at least completed residence work for the latter degree, are eligible to apply. Three such fellowships were provided in 1951-52; and a similar award for 1952-53 has now been made to Mr. John M. Hirschfield of the University of Chicago. Inquiries relating to 1953-54 should be made of the Director, 1900 East Monument Street, Baltimore 5, Maryland, preferably before December 15, 1952.

The Institute also announces the appointment of Dr. Ludwig Edelstein of the Johns Hopkins University, as Lecturer in Greek Medicine; the appointment of Dr. Barkev S. Sanders, formerly Chief of Division of Health and Disability Studies, Social Security Administration, as Lecturer in Medical Economics; and the reappointment of Dr. Alexandre Koyré, of L'Ecole Pratique des Hautes Etudes, University of Paris, as Visiting Professor in the History of Science for the first semester, 1952-53.

NATIONAL NEWS

American Academy of the History of Dentistry

The following tentative program for an annual meeting at St. Louis, Mo., September 6, is being announced by Harold L. Faggart, D. D. S., Secretary, 2120 Pine St., Philadelphia 3, Pa.

9:30 a. m. · Address of Welcome: President of the Academy
Response: Vice President
Presidential Address: J. Ben Robinson

12:30–2 p. m. Luncheon. Address: L. Laszlo Schwartz

Afternoon: Business Meeting.

Armed Forces Institute of Pathology

An exhibit covering ninety years of its history and including relics of Walter Reed's yellow fever experiments will be displayed by the Institute (9th and Independence Ave., S. W., Washington, D. C.) for six months subsequent to May 21. (See *J.A.M.A.*, vol. 149, p. 958, July 5, 1952.)

Philadelphia

Dr. George Sarton's lectures as A. S. W. Rosenbach Fellow in Bibliography at the University of Pennsylvania have been postponed from October, 1952, until January, 1953. His three lectures on "The Appreciation of Ancient and Medieval Science in the Renaissance" will be heard on January 16, 23 and 30, 1953. They will deal in turn with medicine, philosophy and mathematics.

Personal

Hyman I. Goldstein, M. D., of Camden, New Jersey, Honorary Member of the Third Pan American Congress of Gastroenterology and First National Mexican Congress of Gastroenterology, Mexico City, May 1952, and an official delegate to the Congresses of the National Gastroenterological Association, addressed the congresses on "Some Historical Aspects of Hepato-biliary Diseases" and on "Some Historical Aspects of Diseases of the Pancreas." Dr. Goldstein also spoke on "Some Historical Aspects of Cardio-vascular Diseases and Congenital Anomalies" before the National Mexican Society of Cardiology at the National Institute of Cardiology (Mexico) of which Professor Ignacio Chavez is the Director. Dr. Goldstein addressed the physicians of Acapulco, Guerrero, Mexico (May 1952), on "Historical and Clinical Aspects of Some Biliary Tract Diseases."

ERNA LESKY. *Die Zeugungs- und Vererbungslehren der Antike und ihr Nachwirken.* Akademie der Wissenschaften und der Literatur. Abhandlungen der geistes- und sozialwissenschaften Klasse, 1950, No. 19. Mainz. 201 pp.

This account of ancient Greek "genetics" includes, first of all, the Pythagorean Alkmaion of Croton. He derived the semen from the brain, which he regarded as the central organ ("Hegemonikon") of the body (*encephalo-myelogenic theory*). Alkmaion's theory was formerly thought to be the result of empirical research. It is, however, largely speculation on the analogies between macrocosm and microcosm which seems to have inspired it—speculation that derives from the Irano-Persian documents brought to light by Albrecht Götze.

In the Hippocratic Corpus traces of the encephalo-myelogenic theory appear to be connected with the Hippocratic anatomy of the vascular system. The Timaeus regards the "medulla" as the centre of the generative faculties and Galen, commenting upon this, shows himself well acquainted with "this old theory." It was largely superseded by the *Pangenesis-theory*, as held by the Atomists. In this, semen was derived from all parts of the body.

The repercussion of these theories is shown, for example, in the views held on sex determination. Originally, this was thought to depend upon the prevalence ("Epikrateia") of the male part or of heat or cold, or else on the location of the seed in the right or left part of the uterus; in the atomistic theory it is attributed to the prevalence of a certain portion (atom-complex) of substances in which male and female partner shares equally. The Hippocratic authors largely accepting the Pangenesis-theory, shift the emphasis from the physical unit, the atom, to the humours (notably mucus and bile). At the same time, strict preformation in the atomists' sense gives place to the inheritance of a more general material structure. In this, the Pneuma, i. e. an active force acting on matter (epigenesis) is given an important function. Much of what is nowadays regarded as purely hereditary is now dissolved into milieu—conditioned, exogenous, factors. For example, in twins, the space available in utero, modification in nutrition of the embryo by the particular nature of menstrual blood, and the simultaneous time of birth are looked upon as genetic factors. On the other hand, the treatise on epilepsy clearly shows that hereditary transmission of disease was realised. The same applies to phthisis and other conditions.

Aristotle's doctrine has been rightly discussed against the background of his general ideas notably those of matter and form. In spite of his general view of the female as the passive element that receives the active generative impulse from the male, he explains inheritance of maternal features in terms of active impulses deriving from the female germinal substance (the "catamenia")—an inconsistency which aroused Galen's objections. The book concludes with a discussion of the school of Alexandria, of the Stoics and of Galen. The latter discovers the analogy between male and female sexual organs and, following from that, the significance of the "female semen."

The presentation of the complicated development of the ancient doctrines is detailed, critical, and thorough. In addition, according to its title, the book purports to deal with the "Nachwirken" of the ancient doctrines in later times. There are even comparisons with present day genetics. Such digressions, based as it seems on admiration of the achievements of a period in which the author happens to live, might be found somewhat laboured and gratuitous. On the whole, the passages on the after-effects of the ancient theories are sketchy. For example, there is no discussion of the influence of the ancient doctrines on Aristotelian and Anti-Aristotelian naturalists of the Renaissance. Not more than a few passages are devoted to Harvey and in these a modern book is extensively quoted in which Harvey's deep and striking Aristotelian leanings are strangely and unduly minimised.

The present treatise is really a deep-searching history of the Greek doctrines and therefore most welcome as an important reference work for the historian of medicine and philosophy.

WALTER PAGEL

LAWRENCE BABB. *The Elizabethan Malady*: A Study of Melancholia in English Literature from 1580 to 1642. East Lansing: Michigan State College Press, 1951. ix + 206 pp. $3.50.

It is not easy to give a clear and thorough account of the vast confused body of Renaissance scientific theories of physiology and psychology uniting in the concept of melancholy. Nor is it easy then to apply these theories to the elucidation of many hundreds of characterizations in the English drama and prose fiction of the age. But Professor Babb has accomplished both these difficult tasks with a scholarly soundness and sanity of approach which will surely make his book standard in the field.

In outlining the Renaissance scientific theory of melancholy he properly stresses that it rested in the main Galenic physiology. Mental conditions were thought to be traceable primarily, though not wholly, to physical origins in the humors. Despite much vagueness in Renaissance usage, melancholy might be defined, he thinks, as "a psychosis caused by a black bilious humor and characterized by morbid depression, continuous or recurrent" (p. 37). It differed from the natural melancholy complexion more in degree than in kind. Likewise there was no discoverable line of distinction between melancholy and madness, the difference, if any, being again one of degree. Whatever its degree, melancholy in this Galenic view produced emotions of fear and sadness which made the sufferer's character dull and lethargic. But as against this medical tradition, Babb clearly points out, there was also a Renaissance conception, going back to Aristotle, that melancholy men were brilliant and imaginative (p. 66). The two conceptions were hopelessly intertangled in Renaissance thought and literature, often in direct opposition to each other, sometimes satisfactorily reconciled in the Elizabethan characterization of the malcontent.

By way of transition to the ensuing chapters on melancholy in English Renaissance literature, Babb then discusses very sensibly the question of how much the

authors and their readers of the period knew about such scientific theories. Authors were, he believes, unmistakably affected by the prevailing physiological psychology. But they were not specialists in it and did not assume a specialist's knowledge on the part of their audiences. Even writers who did not read much in science probably picked up at least the main outlines of scientific theory from their colleagues or elsewhere. Melancholy as portrayed in the literature of the age therefore was science simplified and popularized. certainly not orderly or consistent. These conclusions are cautious common sense. There is no effort to make all Elizabethan and Stuart writers appear completely scientific, but the position is decidedly taken that by and large fictional characterization, especially in drama, was much influenced by science.

The chapters on these literary characterizations show very extensive and careful reading. Melancholic characters are divided into three main groups: the malcontents, those suffering from various forms of pathological grief, and the love sick. Professor Babb traces the literary vogue of melancholia in England to travellers who brought it home in the 1580's from Italy, where Ficino and others followed Aristotle in regarding it as a malady of great minds. The disgruntled or seditious traveller was, he thinks, the original melancholy malcontent. Within the malcontent group the primary type was the man who resented the world's neglect of what he considered his superior abilities. Other derivative types were the melancholy villain, the melancholy cynic, and the melancholy scholar. Many examples of each type are discussed and their main traits cogently analyzed. Of special interest is the suggestion that the melancholy villain of Elizabethan drama derives from the association of melancholy with criminal propensities in scientific theory. Among those afflicted with pathological grief Hamlet stands out. Professor Babb believes that the Elizabethan audience saw him as a definitely pathological, melancholy character and interpreted the problem of his delay as a subjective one arising from the inability of melancholy men to act decisively. Several chapters are likewise given to discussion of characters affected by love melancholy, the conception of which came partly from medical and psychological science, partly from the literary tradition of courtly love. A concluding chapter summarizes the juxtaposition of the Galenic and Aristotelian concepts in the life and literature of the age, using Milton's *Il Penseroso* as a fine expression of the Aristotelian.

Where so much is given so admirably it seems ungracious to ask for more. I think it a pity, however, that in outlining Renaissance scientific views of physiology and psychology Professor Babb did not give us the benefit of his reflections on the philosophic and religious implications of the situation. We have had other outlines of these sciences, though none better than his, but we lack good discussions of the metaphysics of the Renaissance concept of how soul is joined to body, how they function together in melancholia as well as other conditions, what are the relative spheres of medicine and psychology, what is the function of divine grace, where free will ends, how far the manners of the soul follow the temperature of the body, and the like. Answers to such questions would in themselves, I believe, throw much light on literary characterizations in the drama and elsewhere. One subsidiary point worth stressing perhaps is that physicians and surgeons like Wirtzung and Lowe writing strictly medical books pay very little attention to the soul in

melancholia, as distinguished from physicians like Bright writing on what we would consider psychology, who of course pay a great deal. In passing one might also suggest that the literary vogue of melancholia in England should not be attributed too exclusively to Italian travel in the 1580's, since such travel had been going on for a long time before. The fact that by this time Elizabethan drama had evolved into readiness for more realistic psychological portrayal probably had something to do with it.

Great labor has evidently gone into this book. The detail is immense, the style terse and clear. Professor Babb seems to have read everything and read it wisely. We can accept his findings with confidence and gratitude.

PAUL H. KOCHER

CHALMERS G. DAVIDSON. *Friend of the People: The Life of Dr. Peter Fayssoux of Charleston, South Carolina.* Columbia, S. C.: The Medical Association of South Carolina, 1950. vii + 151 pp. $2.75.

Peter Fayssoux (1745-1795) of Charleston was one of that interesting and important group of young Americans who studied medicine at Edinburgh in the generation before the American Revolution and returned home to practice, establish medical institutions, and play the part of philosophers in the New World. Fayssoux was in Charleston what Morgan and Rush were in Philadelphia, Samuel Bard in New York, Walter Jones in Virginia; and he deserves to have a biography.

This small book presents probably all that is or can be known about Fayssoux. It is not much—not nearly as much as we wish—and because of this paucity of biographical materials Fayssoux never quite comes alive as a distinct, individual person. Had he written more or preserved the letters he received, his biographer could present him in all the fullness of a many-sided career. For Fayssoux was certainly typical of his time and place. Son of a baker, born into a Charleston which was settled and prospering, he never knew hardship or want. At Edinburgh, where he was graduated in 1769, he was a friend of Benjamin Rush, with whom he corresponded until his death. In the Revolution he was a surgeon with provincial troops at Fort Moultrie in 1776 and chief physician of the hospital in the Southern Department three years later. After the war he was interested in internal improvements and active in politics, being a mild anti-Federalist. By studying the files of the Charleston *City Gazette* for 1788 Professor Davidson has thrown new light not only on Fayssoux' role in the South Carolina ratifying convention but on the larger question of Federalism and anti-Federalism in South Carolina. Though he leaned to the republican side in politics, Fayssoux was no radical. He voted to move the state capital from Charleston on the coast to Columbia in the interior; but he was against reapportionment, which would have been a substantial recognition of the western counties. If he was a "friend of the people," he was certainly a moderate one.

To physic and politics Fayssoux added the life of a planter. He purchased a confiscated Tory estate and, though usually deeply in debt, owned a nine-room house in Charleston and a rice plantation with 72 slaves in St. John's Parish.

This is the story Professor Davidson has told in a matter-of-fact way. He has culled materials from a wide variety of sources and has indicated clearly what those sources are. The medical historian will regret that among the material for Fayssoux' life there was so little about his medical practice. His biographer is able to tell us about Fayssoux the politician, Fayssoux the planter, Fayssoux the Charleston citizen (there is an interesting chapter on Charleston's sympathies with the French Revolution); but he can tell us little about Fayssoux the physician. Except for the minutes of the Medical Society of South Carolina, of which Fayssoux was a founder and the first president, and for some letters on medical cases, there was nothing about Fayssoux' medical practice. Unless their subject was so unlucky as to become involved in a public controversy over some point of medical practice, as Rush did in the yellow fever epidemic, all biographers of eighteenth century American physicians seem to encounter the same lack.

In the history of medicine in the United States Peter Fayssoux was not a great man. But this sketch of his life will help us understand more exactly just what American physicians in the eighteenth century were. Though not indexed, the book is annotated and has a full bibliography.

WHITFIELD J. BELL, JR.

L. H. BUTTERFIELD (editor). *Letters of Benjamin Rush.* 2 vols. Published for the American Philosophical Society by Princeton University Press, 1951. lxxxvii+1295 pp. Ill. $15.00.

For more than a century a distorted picture of Benjamin Rush has been accepted by posterity. Its correction, largely in the present generation, by such works as N. G. Goodman's *Benjamin Rush—Physician and Citizen* and Corner's richly annotated edition of Rush's *Autobiography* will now receive added impetus from this two volume edition of Rush's *Letters.*

It contains more than 650, extending from 1761 to 1813, the year of his death, and yet they represent but a small percentage of the total. Truly Rush was a "major letter writer in a letter writing age." The letters represent less than half of those thus far located, and two thirds of them are said to have never before been published. They offer an unequalled opportunity to learn at first hand about this eminent and versatile man of violent contrasts. You find him vituperative yet philanthropic and a lover of people, indiscreet but always sincere and intellectually honest, opinionated to a degree, yet observant and not infrequently modifying his views as a result of his observations; with a capacity for friendship only surpassed by his ability to make enemies. He was above all a true physician because he loved his profession, yet adhered so strenuously and vociferously to false principles (such as excessive bleeding and purging) in a period of biological ignorance, to be sure, that his therapy is still a frequent target for ridicule. Such contrasts and more are to be found scattered through his correspondence and in Butterfield's all too brief Introduction of 13 pages.

The letters chosen for publication here were taken from more than 60 sources, the provenance being given after each letter. The most frequently used collections

were the Adams Manuscript Trust, the Library Co. of Philadelphia, that of the late Dr. Josiah Trent, who bought many at the dispersal of the Alexander Biddle papers in 1943 and offered them with the rest of his Rush material for use in the preparation of these volumes. The most numerous in the selection are to his wife and to John Adams, but prominent physicians, statesmen and philanthropists, both here and abroad, appear in the Table of Contents in large numbers. The 48 pages that this Table consumes are all the more valuable in indicating in a line or two the chief topics contained in each letter.

The *Letters*, which appeared over a period of 53 years, are presented in strictly chronological order. They have been divided with but little inconsistency into 6 groups: the earliest covers the longest period, 14 years, up to the Revolution, but it also occupies the fewest pages; the second, "Love and Patriotism," carries through the Revolution, occupying 228 pages; the third, "a Multitude of Causes," covering from 1784-1792 and requiring 308 pages, is the longest in the book; the fourth covers the 8 yellow fever years in 204 pages, the fifth takes 154 pages for 9 years of "Literary Fame and Domestic Tranquillity"; while the sixth, "Last Years," oddly enough consumes 200 pages for 5 short years. Rush was active to the end, relatively long letters to John Adams and Samuel Miller have been written within 10 days of his death from an alleged typhus fever. This occurred when he was at the very peak of his fame, when he was widely recognized as one of the few Americans of international importance as well as the leading American physician of his time.

One may well believe that physic (i. e. medical theories and practice, aphorisms, controversies with physicians and so on) played an even greater role in Rush's correspondence than appears in these volumes. The Editor's selection was apparently rigorous as it included samples only of Rush's theories of disease, his special forms of treatment, as in his letter to Jefferson (p. 856), his avoidance of correspondents' request for advice without pay (p. 810) and many more, rather than full presentations of any given fields. The correspondence with his wife and with John Adams are notable exceptions to the method of selecting only samples. Obviously, among the ten thousand manuscript items in the Ridgway collection there must be many giving simple medical advice, and there must have existed many more received and soon destroyed. Rush's unitary theory of the cause of fever—and hence of all disease—can be traced from 1790 (in his letter to de Rosset on the proximate cause of fever being due to a "convulsion" in the arterial system) and the letter of 1791 explaining why some sprains are to be treated with cold others with hot applications. This letter to the Rev. Jeremy Belknap also indicates his objections to treating diseases that had names assigned to them rather than "suiting our remedies to certain conditions of the system"—a logical prelude to his later view that the arteries were the sole cause of disease. To John Redman Coxe, one of the two of his assistants who survived the 1793 epidemics, he advocates (1795) the "state of diseases," rather than a nosological arrangement, and claims that "the lancet has at last become less unpopular in our city." When yellow fever returned to the city in 1797, Rush continued to salivate and venesect when the condition of the pulse so indicated, and reiterated (p. 791) that fever was a unit which governed treatment by its force. An exposition of his "new system of medicine" was given in his

farewell lecture to the University students and part of it transcribed in a letter to John Adams (p. 915). In it his opposition to the system of his "very dear Master," Dr. Cullen, must refer to the latter's giving a prime role in disease to the nervous system, whether in spasm or atony (exaggerated or weakened function). It is to be hoped that the rest of this address, empty and even absurd to modern eyes, is not a fair sample of Rush's lectures in general, and it was probably included as another example of Rush's contrasting characteristics. For those interested in obsolete medical theories, the intricacies of the Cullen, Brown, and Rush "systems" may be seen beautifully set forth in an Appendix in Corner's edition of Rush's *Autobiography*.

Those letters that have been included to illustrate concrete advice to patients are full of good advice, based on observation and reading, that would be largely accepted as sound therapy today. Rush's accord with Franklin on the nature of the common cold (p. 80) was the basis for some common sense therapy, though it is difficult to agree with the Editor's suggestion that Rush's observations constitute a shadowy anticipation of modern concepts of allergy. Reasonable and "modern," too, are his suggestions for the treatment of chronic respiratory disease, such as travel, a high dry climate, and moderate exercise. They also appear in his recommendations to Thomas Smith (p. 631) for handling his wife's chronic "complaint of the breast" (consumption). Incidentally, though Rush thought her in a critical situation, happily she lived for another 19 years. The advantages of riding on horseback, which he especially emphasized, he obtained from the revered Sydenham, after whom he named his summer estate. Rush's early and enthusiastic adoption of vaccination (1802) is evidenced in several letters, and especially those to Jefferson to whom he appears to have given credit for naming the procedure.

Of Rush's medical services to medicine, none stand higher than his efforts to improve the treatment of the insane, which were his concern for fifty years. These alone are sufficient to refute Victor Robinson's ill considered remark that Rush contributed nothing of value to medical progress and to support those who regarded him as the leading American physician of his time. These *Letters* include a number to the Managers of the Pennsylvania Hospital, culminating in an epitome, in 1810 (pp. 1063-66), on such matters as solitary buildings, separate floors for the sexes, material comforts (including a water "close stool" instead of the fetid chamber pot), occupational therapy, specially trained attendants to share in the patients' diversions, exclusion of sightseers, and so on. Believing that insanity was, like all others, a disease of the arteries, he of course advocated severe bleeding and, as he always saw what he was looking for, he believed that he "cured" many. Even his gyrater (p. 961) and Inquisition-like tranquillizer (p. 1059)—a strong armchair "with several appropriate peculiarities"(!) were invented to aid the cerebral circulation, respectively in torpid and maniacal forms of insanity. His advocacy to Lettsom (p. 443) of warm and cold baths, which he playfully refers to as his fondness for a voyage to Otaheite (Tahiti), would meet with more approval today.

Rush's success as a medical lecturer and teacher of clinical medicine becomes evident in these volumes by indirection, in his letters replying to former students and apprentices and as occasional remarks in letters to family and intimate friends.

However, here in gauging his success as a teacher, one must rely chiefly on what his students, eulogists and historians have said about him, and the fact that "he taught more students than any contemporary American professor of Medicine." Letters to the Trustees of the University show how far his views on the proper curriculum were in advance of his times: for instance, 18 months attendance spread over 3 years (as at Edinburgh) instead of the current 8 months in 2 years; addition of courses on natural philosophy and rural economy (not adopted). We read in the Adams series of an introductory lecture in 1807 on veterinary medicine, said to be the first given in this country, also others on pathology and physiology, diseases of the eyes and ears, medical jurisprudence, and in 1812, the year before his death, on "Duties which physicians owe to each other." Dissensions among physicians, which he regarded as the most rancorous of any, were well illustrated in his own life, as he recognized in his letter tc his son James (1810); but there is no mention of his own contribution to a situation in question.

Rush's hostility to George Washington during much of the Revolutionary period is perhaps the most widely remembered item of his career by the average non-medical person. It was even thought by some to have led him to join the Conway Cabal, an opinion that was fostered by the unfortunate reluctance of his descendants to put all the evidence before the public. These *Letters* not only include Rush's letter to Patrick Henry—"the rashest act" of an indiscreet life—and a later apologia (1812) in a letter to John Adams, but Butterfield also gives his evaluation of the whole matter in a twelve page appendix. There seems good reason to accept this, like the chapter in Goodman's *Benjamin Rush* (1934), and conclude that in the difficult 6 months of 1777-78 Rush wanted to see Gates replace Washington from patriotic, if mistaken, military motives and that he did not conspire against the Commander-in-chief. Even his unsigned letter to Patrick Henry has been explained as not anonymous according to the dictionary's second definition of "unknown authorship." Not only was omission of the signature a common custom, that Rush occasionally followed, but also the views expressed and the undisguised handwriting were at once recognized by both Patrick Henry and Washington as belonging to Rush. Though often a good hater, Rush was soon on friendly terms with Washington, as seen in various references in these *Letters* [Cp. in 1779 "our great and good General Washington" (p. 237); in 1780, Washington's invitation to dine at Headquarters; the anecdote about Washington not deserting his stage companions (p. 330); the exchange of letters on Rush's gift of some mangel wurzel seed (p. 459); and so on]. This was not the only quarrel that Rush outlived, though as late as 1812 he referred to "a bit of gossip passed on at third hand by Adams to the effect that Washington had once said '. . . Dr. Rush was the most black hearted scoundrel he had ever known'."

Reference to another of Rush's better known quarrels, with William Cobbett of Rush-Light fame, is made in a number of these letters, and the whole subject is reviewed by Butterfield in a 6 page appendix.

Rush's correspondence with his wife pervades both volumes. Beginning early in 1776, their wedding year, it reaches its peak in 1793, when they were separated by the yellow fever epidemic. For that one year volume 2 contains 58 letters to his beloved wife. From August to November references to the epidemic are so

numerous that one can derive a vivid picture of the effect of the disease on Philadelphia from them alone. An excellent "short account of the origins, symptoms and treatment of the fever," (p. 794) was given by Rush to his friend John B. Rodgers, Professor of Midwifery at Columbia, and also published as a means of relieving the strain on his private correspondence. Incidentally, the letters selected for 1793 are more than twice as numerous as for any other year in the series. When Rush himself was attacked by the fever "for the third time," * October 10th, and almost died, he succeeded in writing to his wife every day so as not to alarm her, as he wrote his sister-in-law, and did not inform her of its seriousness till the epidemic had past. Numerous letters to most all of his large family, as well as tender messages to them in letters to his wife, and the Adams correspondence reveal an unexpectedly strong amiability in his character.

Space does not permit notice of the documentation that these *Letters* offer of many of the facets of this entertaining character, such as his changing religious views and affiliations, his domestic happiness, his philanthropic pleading for the abolition of slavery and of liquor (but not wines), his impatience with inaction, his untiring energy in the persistence of his beliefs, his incautious and uncritical castigations of friend and foe. Some of these, however, should not be taken too seriously, but rather as examples of his use of correspondence to unload his overactive, plethoric mind. Of his reverend master, John Redman, for instance, to whom he was apprenticed for 6 years before going to Edinburgh, he complained to Morgan (1768) that Redman never taught him anything and didn't even answer letters. Yet not long after he was Redman's patient (1778), also was asking Redman to take charge of a patient, and eventually wrote a highly eulogistic obituary about him.

It is good to read that a Benjamin Rush Union Catalogue is planned to be kept in the Library of the American Philosophical Society. Like those already in operation for Jefferson, Lincoln, and Theodore Roosevelt, it "envisions a permanent repository of all information relating to its subject."

It would be difficult to exaggerate the excellence of the editing of this outstanding production. Dr. Butterfield, who is now Director of the Institute of Early American History at Williamsburg, Va., began this task while at Franklin and Marshall College under a grant from the American Philosophical Society and continued it in connection with his associate editorship with Julian Boyd of the 52 volume *Papers of Thomas Jefferson*. Well trained, saturated with 18th century American history, and with high standards of scholarship, he will, we may hope, continue the valuable historical contributions of which these *Letters of Benjamin Rush* are a conspicuous sample.

EDWARD B. KRUMBHAAR

JOHN M. GIBSON. *Physician to the World. The Life of General William C. Gorgas.* Durham, N. C.: Duke University Press, 1950. ix+315 pp. Ill. $4.50.

William Gorgas, an authentic giant of American medicine, was born in Mobile

* This is not acceptable by modern standards, and two of the times are to be explained either as relapses or as incorrect diagnoses. The diagnosis of yellow fever on purely clinical grounds may often still be a difficult one.

in 1854. His father was an Army officer and later became Chief of Ordnance in the Confederate Army. The child was attended at birth by Dr. Josiah Nott, a famous protagonist of the idea that yellow fever is transmitted by mosquitoes. Young William witnessed the attack on Fort Sumter in 1861 and the burning of Richmond in 1865. In 1870 he saw an outbreak of yellow fever in New Orleans. This inspired him with a youthful hope "to try to find something that will drive this terrible thing from the earth." At the same time he also yearned for an Army career. Denied admission to West Point, he elected to study medicine, hoping in this way to gain entrance into the armed forces.

In 1879 he was graduated from Bellevue Medical College in New York and after a year's internship entered the army. His early interest in yellow fever was stimulated by frequent contact with the disease at military posts in Florida and Texas; he himself suffered an attack of the disease in 1882, as did Miss Marie Doughty, the young lady who later became his wife.

By this time Gorgas' preliminary studies of yellow fever had begun to attract the attention of military circles. He was well acquainted with Dr. Nott's hypothesis concerning the disease but he rejected it. During the Spanish-American War he was placed in charge of the yellow fever camp at Siboney. In 1898 he was made sanitary officer of Havana.

Convinced that yellow fever was caused by filth and not by mosquito bites, Gorgas instituted a thorough cleansing of the city. This measure was followed by abrupt reductions in the incidence of typhoid fever and dysentery and in the general death rate. Yellow fever increased, especially in parts of the city inhabited by the wealthier classes. Gorgas still refused to believe in mosquito transmission of the disease, despite the arguments of Carlos Finlay. He later wrote

> I can recollect in those years of our very pleasant and cordial relations having spent a good many hours trying to show Dr. Finlay the absurdity of his mosquito theory of the transmission of yellow fever.

At this juncture Walter Reed and his immortal colleagues arrived in Cuba and demonstrated conclusively that Finlay was right.

Gorgas then attempted to control yellow fever by subjecting non-immune persons to the bites of infected mosquitoes. This effort caused 3 deaths among 16 subjects and was promptly discontinued. Control by isolation failed also. Gorgas then instituted a direct attack against the Aedes mosquito and was promptly successful. Historians have failed to recognize that Gorgas' campaign against the Aedes was in effect "species sanitation," a technique which is usually credited to Sir Malcolm Watson because of work done about ten years later against malaria in Malaya. It is also worth noting that in Havana death from malaria was nearly six times as common as death from yellow fever. Gorgas' campaign against malaria was an outstanding success but was necessarily less spectacular than his yellow fever work.

In 1902 Gorgas was recalled to the United States in order to prepare for work on the Panama Canal. The first Canal Commission consisted of six civil engineers headed by a retired admiral. Despite the efforts of the American Medical Association, Gorgas was not included in the Commission. Instead he was given the secondary post of Chief Sanitary Officer and very little power. The authorities

greatly underestimated the magnitude of the sanitary operations that were necessary. The work was greatly impeded by administrative inefficiency and was heavily entangled in the red tape for which the American government is famous. Even more important was a circumstance which the historian will note with a smirk or a sigh, according to the cynicism or saintliness of his temper, viz. that the Chairman of the Canal Commission and the Governor of the Canal Zone refused to believe that yellow fever is transmitted by mosquitoes! This notable example of resistance to new ideas almost wrecked Gorgas' entire effort. With the arrival of large numbers of non-immune persons the incidence of yellow fever rose alarmingly and workers began to flee.

At this juncture the American Medical Association on its own initiative sent Dr. Charles A. L. Reed to Panama to conduct an independent investigation. The resultant report, which deserves to be regarded as a minor classic in the history of American medicine, impelled President Theodore Roosevelt to force the resignation of the Canal Commission. The new commissioners, like their predecessors, were opposed to the mosquito theory of yellow fever transmission. In fact the chairman attempted to have Gorgas replaced by an osteopath.

President Roosevelt now turned to William Welch and to Dr. Alexander Lambert for advice. The discussions resulted in confirmation for Gorgas and a rebuke to the Chairman of the Commission. After this the work of sanitation proceeded more expeditiously. It was not until 1907 that Gorgas was made a member of the Canal Commission.

The story of Panama Canal sanitation has many aspects which deserve the attention of the historian. The first of these has to do with the development of knowledge of yellow fever. Significant elements in this historical pattern are: the prolonged rejection of Josiah Nott's correct concept of mosquito transmission; the rejection of Carlos Finlay's ideas *and of his evidence*; and the rejection of the mosquito-transmission hypothesis by at least two Panama Canal Commissions despite the conclusive experiments of Walter Reed and the famous work of Gorgas in Havana. These instances all fit together into the well-known historical pattern of the opposition to new ideas. Opposition to sanitary planning occurred in World War I and was by no means absent in World War II.

Another consideration of interest to historians and physicians has to do with the failure of the French to complete their canal. It is a standard theme in American folklore that de Lesseps failed because of neglect of sanitary measures, whereas the American success was due to superior sanitation. However the studies of Gerstle Mack have shown that the failure of the French enterprise is more probably attributable to inadequate capitalization, financial maladministration, and the ill-advised decision to build a sea-level canal. Thus one more piece of folklore goes the way of George Washington's cherry tree.

In Mr. Gibson's pleasant volume the biographical and historical facts are set forth in a clear and interesting manner. The background is described in adequate detail but without a distressing excess of minutiae. In consequence the principal figure emerges in reasonable verisimilitude. The treatment throughout is notably free from chauvinism. On all counts *Physician to the World* is to be recommended as a useful study of an important historical personage.

SAUL JARCHO

THE BULLETIN RECENTLY RECEIVED *

Paul E. Bechet. *A History of the American Dermatological Association in Commemoration of Its Seventy-Fifth Anniversary, 1876-1951.* New York: Froben Press, 1952. 392 pp. Ill. $10.00.

Sir Russell Brain. *The Contribution of Medicine to Our Idea of the Mind.* The Rede Lecture for 1952. New York: Cambridge University Press, 1952. 30 pp. $.50.

R. C. Brock. *The Life and Work of Astley Cooper.* Edinburgh and London: E. & S. Linvingstone (Baltimore: Williams and Wilkins), 1952. viii + 176 pp. Ill. $4.50.

William Brockbank. *Portrait of a Hospital, 1752-1948.* To Commemorate the Bi-Centenary of the Royal Infirmary, Manchester. London: William Heinemann, 1952. x + 218 pp. Ill. 25s.

F. F. Cartwright. *The English Pioneers of Anaesthesia (Beddoes, Davy, and Hickman).* Bristol: John Wright & Sons (Baltimore: Williams and Wilkins), 1952. x + 338 pp. Ill. $4.50.

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Robert M. Grant. *Miracle and Natural Law in Graeco-Roman and Early Christian Thought.* Amsterdam: North Holland Publishing Company, 1952. 293 pp. $4.50.

James Gray. *History of the Royal Medical Society, 1737-1937.* Edited by Douglas Guthrie. Edinburgh: at the University Press, 1952. xi + 355 pp. Ill. 42s.

H. W. Janson. *Apes and Ape Lore in the Middle Ages and the Renaissance.* London: The Warburg Institute, University of London, 1952. 384 pp. Ill. £3.3.0.

Ernest Sachs. *The History and Development of Neurological Surgery.* New York: Paul B. Hoeber, 1952. 158 pp. Ill. $5.00.

George Sarton. *Horus. A Guide to the History of Science.* A First Guide for the Study of the History of Science with Introductory Essays on Science and Tradition. xviii + 316 pp. Waltham, Mass.: The Chronica Botanica Co.; New York City: Stechert-Hafner, Inc., 1952. xviii + 316 pp. $7.50.

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* The *Bulletin* reserves freedom of decision as to the publications to be listed in this section.

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